

THE PERCEPTION OF DISTANCE

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PART II. THE EXPERIMENTAL TREATMENT OF THE PROBLEM

I. EXPERIMENTAL PROCEDURE

(1) *Design of experiments.*

It is clear from the discussion in Part I of this paper that it is most important to preserve as far as possible the essential relationships between the different types of data in the spontaneous act of distance perception. All factors connected with distance perception should be studied as far as possible *in situ*. They may be impaired or falsified and the effect of this procedure studied, but as far as possible the normal conditions of distance perception should be preserved, with as much as possible of the relevant data intact. Under these conditions it should be possible to obtain from the introspections of the observers some information as to how far immediate perceptual responses are employed, and as to when they are supplemented and replaced by secondary processes involving inspection and deliberate judgement.

These conclusions have been borne in mind as far as possible in drawing up the scheme of experiment described in this paper. An attempt was made firstly to determine the conditions of functioning of disparity

(1) when it was set in conflict with the other perceptual data, (2) when it was eliminated altogether. Afterwards, the other factors of apparent size relationship, brightness and clearness, and of relation to surroundings, were studied in more or less detail. In two of the three main sets of experiments the surroundings and general setting were kept as normal as possible, and most of the principal factors were allowed to operate. In the removal or falsification of certain specific factors, however, others were inevitably affected, and also it was found necessary to interfere, to some extent, with the subject's view of the surroundings and general setting. Moreover, in the third set of experiments it was decided to supplement the results obtained by the other methods by starting from very simple and narrowly defined conditions and working up to more natural conditions. But it was found that in all these experiments the criticism we have made of previous experimental work was only too well founded. Straightforward, direct perception of distance gave way to inspection and hesitating judgement as soon as the perceptual setting became curtailed or confused to any appreciable extent. And under the simplified conditions certain perceptual factors appeared to assume an artificial prominence and importance. But it also appeared that there was a gradual and continuous transition between the type of distance estimation obtaining under normal conditions and that existing under more artificial conditions. Consequently it was possible to indicate in many cases by a species of extrapolation what appeared to be the probable functional importance of various factors under normal conditions.

(2) *Apparatus and details of procedure.*

The apparatus used consisted essentially, in all experiments, of two test objects, flat grey or white cards of different sizes, which moved on tracks, the one, the standard, in the upper half of the field of vision, the other, the variable, in the lower half. The standard always remained stationary during a single observation, but its position was changed from one observation to the next. In the earlier experiments (series A-F), the variable was driven by a motor, and moved continuously along its track towards or away from the observer, but always towards the standard. The observer was required to stop the motion, in some experiments by saying 'Stop', in others by switching off the electric current to the motor, when the variable appeared to be equidistant with the standard. In no case could the observer see the tracks or the attachment to them of the test objects.

In the later experiments (series G and H), both objects were kept

stationary during each observation, and the observer was required to say whether the variable appeared to be nearer or farther than the standard. An exposure of 3 sec. was given, the lights which illuminated the field being automatically switched on and off at the beginning and end of that period.

In series A–C, the test objects were not enclosed, but were illuminated by daylight; they were viewed through an aperture against the background of a black curtain. The aperture cut off most of the view of the surroundings except part of the side wall of the room, also covered by a black curtain. In series D–H, the test objects were enclosed in a large box, uniformly illuminated, with the inside walls painted white. The test objects were illuminated by lamps which moved with them, and were screened from the observer's direct vision. Thus the brightness of the objects did not alter as they moved forwards or backwards.

The full details of the experimental procedure are as follows:

Series A. Prism experiments. The apparatus is shown in Fig. 1*a*. The test objects, T_1 and T_2 (seen from the side), were strips of grey cardboard 0.5 in. wide. The tracks,

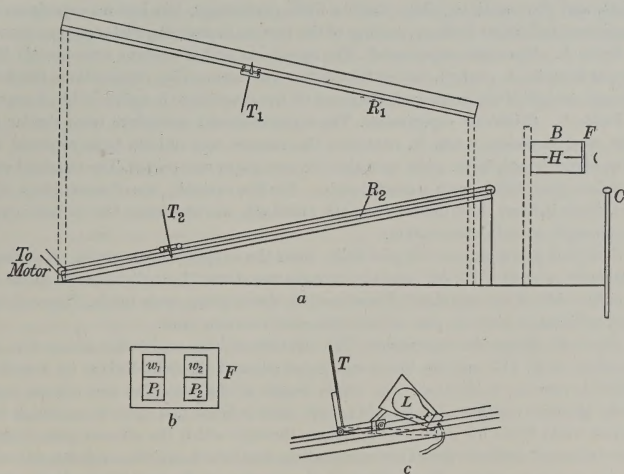


Fig. 1.

R_1 and R_2 , on which T_1 and T_2 moved, were tilted,¹ so that the length of T_1 and T_2 which was visible to the subject remained the same whatever their positions. The

¹ Owing to the tilting, the amount of light reflected by the two test objects was slightly unequal; hence the difference in brightness noted on p. 125.

subject placed his chin on the chin-rest *C* and viewed the field through a rectangular box *B* open at both ends, containing a central partition *H* which divided the field horizontally. In front of *B* was the wooden framework *F*, shown in front in Fig. 1*b*, carrying, in the test experiment, two prisms, P_1 and P_2 , in its lower half and two pieces of plain glass, w_1 and w_2 , in its upper half. In the control experiment the lower half also contained glass. The prisms were each of 1° of angle, so placed as to deviate the field outwards. Consequently, when the binocular images of the variable test object were fused with the same degree of convergence and disparity as those required for the standard object, the former was in point of fact much nearer than the latter to the observer—at about 50 in. instead of at about 85 in.

T_2 moved at approximately 1.4 in. per sec. towards T_1 from a distance away which was varied in random order from 10 to 18 in. in the control experiment. Three standard positions were used, in random order, 80, 85 and 90 in. from the subject's eyes. Thus the average angle of parallax subtended by the standard object was $1^\circ 41' 4''$. In the prism experiment, T_2 had as a rule to be brought much nearer to the subject, and in general was started at positions varying from 35 to 75 in. from the subject. Thirty observations were made in each experiment, test and control. In no case was the subject permitted to see the experimenter adjusting T_1 and T_2 between observations.

Eight subjects took part in the experiments; five of these were experienced psychologists, and the remaining three knew a little psychology, but had no experimental experience, and knew little or nothing of the factors underlying distance perception.

Series B. Monocular experiments. The apparatus and procedure were exactly the same as in series A, control, except that in the test (monocular) observations the field through one half of the framework *F* was cut off by attaching to it a piece of black paper.

Series C. Brightness experiments. The apparatus and procedure were similar to those used in series A and B, controls. The narrow test objects were replaced by pieces of cardboard, $5\frac{1}{2}$ in. wide, on which the grey paper was pasted. The standard was a medium grey, taken from a graded series. For the variable, one of seven greys was used, three lighter, three darker than the standard, one the same; the variable grey was changed at each observation.

Standard positions were 85 and 95 in. from the subject's eyes, giving an average parallaetic angle of $1^\circ 35' 24''$, and the variable was started from distances of 15–20 in. on either side of the standard. Twenty-eight observations were made. Three of the same subjects carried out the experiments, and two new ones.

Series D. Monocular experiments. The test objects were enclosed in a large box, as described on p. 117, and the lower and upper parts of the field divided by a central partition running horizontally the whole length of the box. The test objects were pieces of white cardboard, 6 in. wide, each with a black dot upon it on which the subject could focus his eyes. The apertures through which the objects were viewed were decreased until all view of the surroundings was cut off, and the subject could only see the two test objects. The method of illumination of the test objects is shown in Fig. 1*c*. Each piece of cardboard, *T*, was illuminated by a shielded lamp *L* which moved along the track with it. In addition, two lamps were placed at the farther end of the box, one above and one below the partition, so that direct light from them to the subject's eyes was intercepted by the test objects. These served to keep the amount of light reflected from the sides of the box relatively constant.

The positions of the standard were 36, 40 and 44 in. from the subject, giving an

average parallactic angle of $3^{\circ} 34' 44''$, and the starting positions of the variable were from 10 to 14 in. on either side of the standard positions. Thirty observations were made in each experiment, monocular and binocular. The procedure was the same as in previous series, except that the subject's instructions were made more definite. Thus he was told to stop the movement as soon as the lower surface appeared to be immediately under the upper one, and not to try to equate the distances by equating the sizes of the dots, since these were not equal.

Three of the subjects were the same as those in series A and B, two the same as in C, and there were two new ones.

Series E. Experiments on clearness of surface structure. The arrangement of the apparatus, procedure and subjects were the same as in the monocular experiment in series D, except that in the test observations pieces of grey mottled cardboard, with a black dot upon each, were used instead of white cardboard. Also the subject was instructed to equate the distances by equating the clearness of the mottled pattern on the two surfaces.

Series F. Experiments on surroundings. The apparatus, procedure and subjects were in general the same as those in series D. But in the test experiment the apertures were widened sufficiently for the subject to be able to see part of the walls of the box on either side of the white cardboard test objects. He could also see a small part of the farther end of the box below the upper object and above the lower one. He was directed to try and equate the distances by reference to their setting and relationship to the surrounding walls of the box, but not to try to equate the distances by equating the size of the dots or of the white surfaces, since these were not equal.

Series G. Experiments on size. The apparatus was the same as that used in series D-F, except that the apertures were still further enlarged, to give a more extensive view of the sides of the box. Binocular vision was used.

The procedure was varied in that the test objects remained stationary during each observation. The time of exposure was limited to 3 sec. The lighting circuit was connected to a mercury switch attached to a slowly moving compound pendulum. As the switch tilted with the movement of the pendulum, contact was made between the mercury and the two terminals of the switch, and the lighting circuit was completed; as the pendulum swung back, contact was broken, and the circuit opened.

The positions of the standard object were 32, 34 and 36 in. from the observer, with an average parallactic angle of $4^{\circ} 12' 40''$, and of the variable, from 27 to 41 in., at 1 in. intervals. The test objects were pieces of grey mottled cardboard used in series E. The standard was 4.7 in. wide, and in the control experiment the width of the variable was 5.7 in. In the test experiment the width of the variable was varied with its distance from the standard, as follows:

Variable 4-5 in. in front of standard (angular separation $-32' 24''$ to $-26' 36''$)—width 6.2 in.

Variable 2-3 in. in front of standard (angular separation $-20' 24''$ to $-14' 0''$)—width 5.9 in.

Variable 1 in. in front to 1 in. behind standard (angular separation $-6' 16''$ to $7' 36''$)—width 5.7 in.

Variable 2-3 in. behind standard (angular separation $15' 44''$ to $24' 24''$)—width 5.5 in.

Variable 4-5 in. behind standard (angular separation $33' 40''$ to $43' 28''$)—width 5.2 in.

Thirty-three observations were made in the control, and the same number in the test experiment. In the latter also were made ten observations in which standard and variable were equidistant, but the width of the variable was varied, widths of 5.2, 5.5, 5.7, 5.9 and 6.2 in. being given.

Three of the same subjects carried out these experiments, together with two new ones.

Series H. Brightness experiments. The apparatus and procedure were similar to those of series F, and the subjects were the same. But in the test experiment, the brightness of the variable was varied, instead of the width. The brightness of the standard was kept at a voltmeter reading for the current through its lamp of 90 V. D.C., while the voltmeter readings of the lamp of the variable were varied with the position of the variable (the positions of the variable were the same as in series G): (1) Voltage 100; (2) Voltage 95; (3) Voltage 90; (4) Voltage 86; (5) Voltage 82. In addition observations were made in which standard and variable were equidistant, but the voltage of the lamp of the variable was varied, voltages of 100, 95, 90, 86 and 82 being used. The voltages of the field lamps, at the back of the box, were kept constant throughout.

Series I. Subsidiary experiment. A set of experiments was carried out in which monocular vision alone was employed, and the field of view so greatly restricted that only the test objects were seen against a distant dark background. In the control experiment the standard and variable were viewed through two 0.25 D concave lenses, situated at 2 ft. from the subject's eye, at the end of a darkened tube. In the test experiment, the standard was still viewed through one of these, but the variable through a 1.25 D convex lens. The variable was always at a distance less than the focal length of this lens, so that it appeared as a virtual magnified image. The test objects were horizontal strips of grey mottled cardboard, 0.5 in. wide.

The positions of the standard were 17, 20 and 23 in. in front of the lenses, with an average parallax angle of $3^{\circ} 15' 16''$, and the variable was started from a distance of 10-15 in. farther or nearer than the standard. In this series alone, the subject was allowed to manipulate the movement of the variable. The trolley which carried it was driven by means of a chain drive and sprocket wheels, connected to a crank handle which the subject turned. He was allowed to move it backwards and forwards at will, to the limits of the track on which the trolley ran, until he could find a position in which it appeared equidistant with the standard.

Twelve observations were made in the test, twelve in the control series. Five subjects were used, all picked from those used previously.

(3) *The subjects of the experiment.*

It should be clearly stated that the subjects of these experiments were not selected because they were able to achieve some required level of performance in any of the recognized visual tests of acuity, accommodation or eye-muscle balance. All that was required was that their index of visual acuity should be such that they could perceive readily and discriminate clearly all the test material at any distance used in the course of the experiment, and that if there were any marked abnormality in accommodation or in eye-muscle balance, this should be known. With the

exceptions noted below, no subjects showed such deviation from the others in their experimental performances as to indicate that their visual capacities differed from the normal in any significant way. All the subjects who in their everyday life used corrected vision were allowed to do so throughout the experiments.

However, the results of Subject 11 in series E and I must be accepted with reserve, since he could not clearly perceive details of surface structure under the experimental conditions, such as the grey mottling on the cardboard test objects. He had no difficulty in making the observations in the other series in which he took part (C, D and F), and his results for these series did not differ markedly from those of the other subjects. It should be noted also that the vision of Subject 6 was sufficiently good to give normal results except when the field was diverged by prisms, in series A. Here his results were markedly different from those of the other subjects. This subject was shown by the eye-muscle balance tests to possess a strong left-eye dominance. Consequently under the unusual conditions of this series, fusion was weak and disparity sensations unreliable; possibly the right-eye image was largely ignored or suppressed.

Had any other marked or characteristic variations been observed, and particularly had they persisted in any one subject throughout several series (especially in the control observations) some defect or peculiarity of the subject's visual apparatus might have been suspected. But no such variations appeared. Consequently it seems fair to conclude that the general trend of the experimental results adequately demonstrates the operation of the various data referred to, as they are utilized by the majority of persons in the type of observation investigated.

It should also be noted that throughout this paper no conclusions are derived from any absolute quantitative measures relating to distance perception. Such measures must obviously, to be of any value, be related exactly to precise measures of efficiency of the visual apparatus. In all cases the results of control observations, made under constant experimental conditions, are given together with those for the test observations, and conclusions are drawn from the relations between control and test observations. Thus no claim is made to give accurate numerical measures of the ability to perceive differences of distance (that is, of the differential distance threshold) when, for instance, disparity sensations are functioning, or when they are excluded, or under any other of the experimental conditions described. All that is attempted is to show when and how the factors of disparity, accommodation and the like are utilized in the perception and estimation of distance, and to give a general qualitative

comparison of the extent to which they function in such perceptions. The establishment of precise quantitative relations, both within the range which is called 'normal', and also for various degrees of divergence from it, if it ever prove possible, is still work for the future.

II. EXPERIMENTAL RESULTS

(1) *Experiments on the binocular fusion sensations.*

We may usefully begin the study of the factors connected with distance estimation and their interrelationship by considering the first series of observations, A, in which the field to the two eyes was deviated outwards by inserting two prisms, one between each eye of the observer and the variable test object. Now if the observer relied solely upon the equalization of the disparity sensations, he stopped the movement of the variable object at a distance of about 4 ft.; but if he ignored these sensations (or did not fuse the binocular images), and employed the other perceptual data, he stopped the movement of the variable at the position of real equidistance with the standard, that is, at a distance of about 7 ft.

In Table I are shown the results. In the control observations in which no prisms were used, the average constant error from true equidistance and its standard deviation are given as the difference between the angles of

Table I

Constant error* in distance estimation in difference of
angle of parallax from that of standard

Subject	Mean		Standard deviation	
	Control	Prisms	Control	Prisms
1	5' 4"	2' 0"	1' 42"	1' 4"
2	3' 8"	1' 28"	1' 30"	0' 48"
3	2' 48"	-2' 48"	1' 28"	1' 22"
4	0' 20"	-1' 52"	1' 34"	2' 18"
5	-3' 16"	5' 16"	1' 8"	0' 50"
6	-0' 20"	-20' 32"	1' 34"	3' 52"
7†	5' 52"	-2' 4"	1' 40"	1' 12"
8†	4' 40"	-4' 8"	1' 46"	1' 50"

* + Errors indicate over-estimation (variable placed too near);

- Errors indicate under-estimation (variable placed too far).

This applies to all subsequent tables.

† These subjects were given a rather freer view of the background and surroundings.

parallax subtended by standard and variable; in the observations in which the prisms were used are shown the average constant error, and standard deviation, from equidistance as given by the disparity sensations.

It is immediately apparent that all the subjects except Subject 6 did in fact rely upon the latter sensations in the prism observations. Moreover, the constant error and standard deviation in these observations were frequently less than those in the control observations. Thus in this experiment, in which equidistance and non-equidistance alone were to be estimated, disparity sensations provided an accurate basis for estimation, even though they conflicted with the other perceptual data. It is particularly interesting to note the conflict between them and the sensations of blurring due to faulty accommodation. Thus Arrer's suggestion that the blurred retinal image set up reflex accommodatory movements, in close conjunction with which occurred reflex convergent movements, and that binocular distance estimations were based upon the kinaesthetic sensation afforded by these two sets of movements, cannot hold⁽¹⁾. Clearly, the blurring due to lack of accommodation did not prevent accurate estimation of equidistance from disparity sensations; if any reflex accommodatory movements did occur, they were ignored.

We must, however, qualify these conclusions as to the immediacy of estimation of distance from the binocular fusion sensations. It is clear from the introspections of most of the subjects that they were considerably bewildered by the conflict between the data afforded by these sensations and by the other sensory and perceptual data. This was partly due to the blurring of the images from incorrect accommodation. Again, apparent inequality of size at the position of estimated equality of distance was most confusing, particularly since in the control observations apparent size equality had been used in assisting the equation of distance (the test objects were actually equal in size in series A and B). We shall return to this apparent size factor in the next section. But some confusion also arose from the position of the variable object in relation to the background and from the apparent distance of that background from the observer in the deviated part of the field. Subject 7 reported that he found it most convenient to ignore the background deliberately, and hence avoid the confusion. Thus clearly the process of estimation employed in the prism observations differed considerably from that employed in the control observations, where comparatively normal conditions prevailed. Even when unaware of the nature of the conflict and of the conflicting factors, the observers were bewildered by the confusion and distortion in the spatial relations of the deviated field. But since the variable test object gradually approached the standard from a distance, it was possible for the subject to observe the gradually decreasing degree of disparity

between the two objects, and to report equidistance when disparity disappeared. Thus it is probable that the particular experimental conditions rather enhanced and over-emphasized the use of this factor at the expense of the other conflicting data.

We have already mentioned the abnormal results given by Subject 6. This subject has pronounced left-eye dominance, and it therefore seems probable that in his case disparity only functioned very imperfectly. In fact, he reported that he did not always fuse the binocular images. Thus he tended to ignore the disparity sensations, and to observe the data supplied by monocular vision with the left eye. Of these data, the most important were those supplied by the relationship of the object to its background. Consequently he estimated the position of equality to lie nearer to, that of real equality than to that given by the disparity sensations.

Since under the conditions of series A disparity sensations were in general of such importance in distance estimation, we should expect that the effect of removing them by employing monocular vision would be considerable. We do find in fact that the constant error is usually greater, and the standard deviation much greater, in a set of results obtained in series B with monocular vision than in a control set with binocular vision. The results are given in Table II. The high variability of the

Table II

Constant error in distance estimation in difference of
angle of parallax from that of standard

Subject	Mean		Standard deviation	
	Binocular	Monocular	Binocular	Monocular
1	0' 4"	7' 36"	1' 34"	7' 8"
2	-1' 52"	4' 48"	0' 54"	4' 12"
3	0' 36"	4' 20"	1' 34"	4' 12"
4	2' 32"	1' 40"	0' 58"	5' 18"
5	-3' 16"	2' 12"	1' 8"	2' 20"
6	-0' 12"	-3' 0"	1' 24"	5' 52"

results obtained with monocular vision is particularly noticeable. Moreover, there was very great uncertainty among the subjects, especially among those unaccustomed to making observations of this type, not only as to the accuracy of their estimates but also as to the general spatial relationship of the two test objects. Thus some subjects reported that they were uncertain at the beginning of an observation as to whether the variable object was nearer or farther than the standard—although the distance between them was from 10 to 18 in.—and could not always tell if the variable was moving towards them or away from them. These

reports indicate that the gross spatial relationships of objects cannot be readily appreciated in monocular vision, when the spatial setting is such that the relationship of these objects to their surroundings is not immediately perceptible. The remaining perceptual data were inadequate to form a basis for immediate perception (without secondary judgement) of the *distances* of the objects relative to each other or to the background. Neither was the *position* of the objects relative to their surroundings (for instance, to the side wall) directly perceptible, since the surfaces of the objects and of the surroundings were not contiguous—and indeed were fairly widely separated. Thus it was difficult if not impossible for the observer to fit these objects into any continuous receding spatial pattern. Moreover, the field and test objects were reported from time to time to possess a peculiar 'flat' appearance, indicating that the immediate 'perception' of depth and relief (apart from its estimation) was greatly impaired by the elimination of the disparity sensations.

We may conclude that in these circumstances the observer was in general forced to make some type of secondary judgement based upon one or more of the remaining sets of perceptual data. Of these, apparent size data were most commonly used; and Subjects 4 and 5 reported that they relied on these data almost entirely. Relative apparent size clearly afforded a valuable indication at the beginning of the observation as to the relative distance of the two objects; while a continuous increase or decrease of apparent size of the variable object was interpreted as a movement towards or away from the observer. It is doubtful, however, if apparent size comparison could have produced any accurate equation of distance. It was necessary for one object to be at least 5 in. farther away from the observer than the other to give a difference of 1' in the angles subtended at the observer's eye by the horizontal extents of the two objects; and it was unlikely that any smaller angular difference could be discriminated, under these experimental conditions. Hence other data must have been employed to give finer estimates. Brightness also could not have produced accurate estimates of equality, since the actual brightnesses of the two objects were slightly unequal,¹ but the factor described as clearness of micro-structure (see p. 135) may have been of some assistance. Accommodation may also have been utilized to some extent; that is to say, a blurring of the retinal image of one object when the eyes were correctly focused on the other would have indicated non-

¹ This probably accounts for the large constant error shown by Subject 5 in the control observations, in which he reported that he deliberately utilized relative brightness in making his estimates (see footnote, p. 117).

equidistance of the two objects. This factor was probably of considerable importance in the monocular experiments of Wundt, etc. In these experiments Subject 6 reported that he tried to imagine a vertical straight line drawn between the two objects, that is, at a position of equal accommodation; but that he found that the variable object sometimes seemed to jump spontaneously from one position to another—presumably as accommodation was altered in reflex response to a blurred retinal image. But it also seems clear from the high variability of his estimates that accommodation sensations are of little use in producing consistent judgements unless they are operating in conjunction with some general idea of spatial relationship such as that which was formed deliberately from comparison of apparent size. But they may provide a useful fine adjustment within such a scheme of relationship.

Further data as to the effect of removing disparity sensations are afforded by a series of observations, series D, in which size comparisons and cues from surroundings were largely eliminated. The observer could see only two white screens, each with a black dot upon it, the edges and surroundings of which were concealed. Brightness was kept constant, and was also equal for the two screens. The results obtained in monocular and binocular vision are given in Table III. Clearly Subjects 4, 7, 8 and 10

Table III

Constant error in distance estimation in difference of angle of parallax from that of standard

Subject	Mean		Standard deviation	
	Binocular	Monocular	Binocular	Monocular
4	- 5' 24"	-16' 36"	6' 38"	12' 52"
7	-12' 0"	-19' 16"	13' 34"	14' 0"
8	- 1' 44"	2' 36"	17' 0"	24' 16"
9	18' 24"	3' 52"	19' 44"	22' 12"
10	5' 20"	13' 48"	6' 50"	15' 44"
11	-11' 40"	- 2' 20"	12' 34"	17' 14"
12	- 8' 0"	1' 16"	21' 28"	16' 42"

were definitely more inaccurate and variable in making monocular estimates, but Subjects 9 and 11 were more accurate, and Subject 12 more accurate and consistent. The former were definitely assisted by the disparity sensations, while the latter apparently were not. These conclusions are supported by a comparison of the number of large errors made (over 2 in.). Subjects 4, 7, 8 and 10 made 12, 15, 17 and 6, and 26, 25, 19 and 20 large errors in the binocular and monocular observations respectively; Subjects 9, 11 and 12 made 19, 17 and 24, and 20, 15 and 11 respectively. It seems then that under these extremely simplified condi-

tions, disparity sensations are not necessarily of any assistance in the equation of distance or even in the comparison of considerable differences of distance. Subject 12 in particular must have found difficulty in apprehending correctly the disparity between the two objects; but all the subjects showed higher standard deviations in this series than in series B (compare Tables II and III). Convergence was not infrequently incorrect, for lack of fusion and double images were reported by several subjects. Moreover, the true depth impressions produced by fused disparate sensations largely disappeared.

That it was more difficult to perceive and estimate disparity correctly in this series than in series B must presumably have been due to the fact that in this series the background and surroundings were much more completely concealed from view. We may suppose that in series B the various planes of the surroundings receding backwards or extending forwards from the object upon which the eyes were focused provided a series of disparate sensations gradually increasing as the angle of parallax increased; and into this series the percept of the variable test object could easily be fitted. Pouillard⁽³³⁾ has shown that in the comparison of distance of two figures viewed in a stereoscope, the estimation is greatly assisted by the inclusion of intermediate levels and receding perspective lines between the two figures. The impressions produced by the disparity sensations were distinctly imperfect without these. So also in the experiments in Series D, where the surroundings were concealed, no continuous series of impressions of disparity could be obtained, and the subject was obliged to estimate immediately and directly the degree of disparity between the two objects. This was the more difficult to do in that there was a considerable angular separation between the two objects (at right angles to the line of sight), which prevented him from seeing them simultaneously, one by direct, the other by indirect vision, and necessitated his glancing from one to the other. The consequent time interval during which the eye movements occurred would have helped to impair the accurate estimation of the disparity between the two objects. And in consequence within a certain range of distance between the two objects—which was considerable particularly in the case of Subject 12—disparity was not accurately perceived and the two objects were reported as equidistant.

The difficulty in perceiving disparity may also have been enhanced by the difficulty in accommodating correctly. The observer could only perceive that accommodation was correct from the clearness or blurring of the outline and surface features of the test objects. The white test

objects, with no other clearly perceptible feature upon them except the black dot, provided little opportunity for gauging clearness and hence of accommodating correctly. But this defective accommodation may well have operated to an even greater degree in monocular than in binocular vision. We saw on p. 126 that there was some reason to suppose that accommodation sensations assisted in producing a fine adjustment in equating distances. And in these experiments, in which the available sensory data were greatly reduced, just as in Wandt's experiments, it is possible that they were of more importance than in previous experiments. Again, the factors of apparent size and clearness of pattern and its colour qualities, which seemed to operate even in very simple arrangements of this kind, could not be observed unless the eyes were correctly accommodated on the surface. Thus all observers found it impossible to accommodate upon a mottled grey surface at 3-4 ft. away, in monocular vision, unless there was a black dot upon it. One subject could not accommodate on either the grey or the white surface even when there was a dot. Thus accommodation is clearly a process of importance in the perception of spatial relations. It is essential to any clear visual perception of the nature of objects, their apparent size, clearness and colour, and to the inter-relationship of these qualities; and it may also provide information through the direct apprehension of the blurring of the retinal image at non-equidistance. Even if it is difficult to demonstrate overtly that any direct use is made in normal perception of distance of a perceived blurring of the retinal image (the results we have described so far as indicating their direct utilization have been purely inferential), we may conclude that other perceptual data are fundamentally dependent upon them, and cannot be employed unless accommodation is functioning correctly.

(2) *Experiments on size.*

We must now consider in more detail the factor of apparent size. But it is perhaps advisable first to discuss briefly the relationship between 'real', apparent and retinal size. The term 'real' size presents numerous difficulties; it is here used much in the sense used by Thouless (38). It relates to an abstract idea of the size of an object based upon deductions from the apparent size of the object when it is quite close to the observer, in comparison with the apparent sizes of other near objects; particularly of course of the standard sizes or lengths used in measurement, and also of the apparent sizes of parts of the body, with which are correlated the kinaesthetic impressions of these parts of the body (for instance, the distance through which the arm can be stretched). However, whatever

the exact basis of this idea, clearly it is closely connected with the comparison of relative apparent size of near objects at equidistance, which comparison can be made with extreme accuracy. But once differences of distance are introduced, and hence variations in the sizes of the retinal images, apparent size comparison becomes a much more uncertain matter. The dimensions of the retinal images are of course inversely proportional to the squares of the distances. But it has been shown by Thouless⁽³⁸⁾ and others that an observer, asked to compare the size of two non-equidistant objects, will not compare them in proportion to the ratio of retinal size, or in proportion to the ratio of 'real' size; but will employ a ratio lying between these measures. That is to say, the size of a distant object is perceived to be less than its 'real' size, but greater than its retinal size. In general, under normal circumstances, apparent size is nearer to 'real' than to retinal size; and certain experimenters, notably Beyrl⁽⁴⁾ and Frank⁽¹²⁾, have stated that, up to a considerable distance from the observer, apparent size is equal to 'real' size—a phenomenon known as 'size constancy'. But Thouless's observations show that this is not the case. Moreover, the less clear the subject's perception of the general surroundings and the relative distances of objects, the less the tendency to equate apparent to 'real' size. To this point we shall recur later.

But a further consequence of this approximation of apparent to 'real' size is that a change of size of the retinal image is liable to be interpreted as a change of distance of an object of constant 'real' size; and the nearer the approximation of apparent to 'real' size, the more accurately is the change of distance estimated. In some circumstances an apparent change of distance is perceived even when change of retinal size is produced in some other way than by a real change of distance. This is shown particularly well in the experiments of Hillebrand⁽¹⁹⁾ and Calvarezo⁽⁶⁾. In Hillebrand's experiment an expanding iris diaphragm, viewed monocularly in an otherwise dark room, was perceived to approach the observer, but not to increase in size. Calvarezo found that if two like forms of different size were exposed in rapid succession at different distances, a depth effect appeared independently of the attitude of the observer provided that he saw the two figures as a single figure performing optimal stroboscopic movement from one position to the other. These experiments show then that the following conditions are necessary for the perception of change of retinal size as change of distance:

- (1) That 'real' size was assumed to be constant, and hence that apparent size was also approximately constant;
- (2) That the change of retinal size was sufficiently continuous or rapid

to produce the impression of movement; thus sensations of disparity due to movement may also have been involved to some extent;

(3) That no cues were provided from perception of the relationship of the object to its surroundings that there was no real displacement of the object.

Thus under these conditions the tendency to perceive constancy of the apparent size of an object was much stronger than the tendency to perceive constancy of its apparent distance. This agrees with the findings of Metzger⁽³¹⁾ obtained under rather different experimental conditions, that 'figural' units have the property of constancy, while empty spaces between units, for instance, distances between objects, when not filled by the intervening planes of the surroundings, can change at will.

It is clear that in the experiments of Hillebrand and Calvarezo the phenomenon would not have appeared had the subject been able to perceive that there was change in the 'real' size of the object relative to its surroundings, but no change of distance. In other words, a clear perception of the relation of the object to its surroundings will determine if a change of retinal size is seen as a change of apparent size or approximate constancy of apparent size with change of distance. The experiments of Schur⁽³⁶⁾ on the phenomenon of the 'horizontal moon' show that when this relation is only partly clear, difference of retinal size is seen partly as difference of apparent size and only partly as difference of distance. Weber & Bicknall⁽⁴⁰⁾ also showed that apparent size constancy was more complete in the stereoscopic vision of photographs of distant objects than in non-stereoscopic vision when there was of course less appreciation of spatial relationships. Again, if the information as to the change of distance given by the perceived relationship of the object to its surroundings disagrees with that provided by retinal size, a confusion may occur.

This was demonstrated in series G, in which binocular vision was used and the surroundings were clearly visible, but the 'real' size of the test object was varied without the subject's knowledge. Thus in general he assumed its 'real' size to be constant; only one subject suspected a change of 'real' size. Therefore variations in retinal size might have been perceived as constancy of apparent size with change of distance; and constancy of retinal size perceived as constancy of distance. The variable object was not made to move gradually towards the standard, but standard and variable remained stationary, at different distances apart, and were exposed for a limited period of time. In the control observations, the sizes of the two objects were constant though unequal; but in the test observations the size of the variable was made less when it was

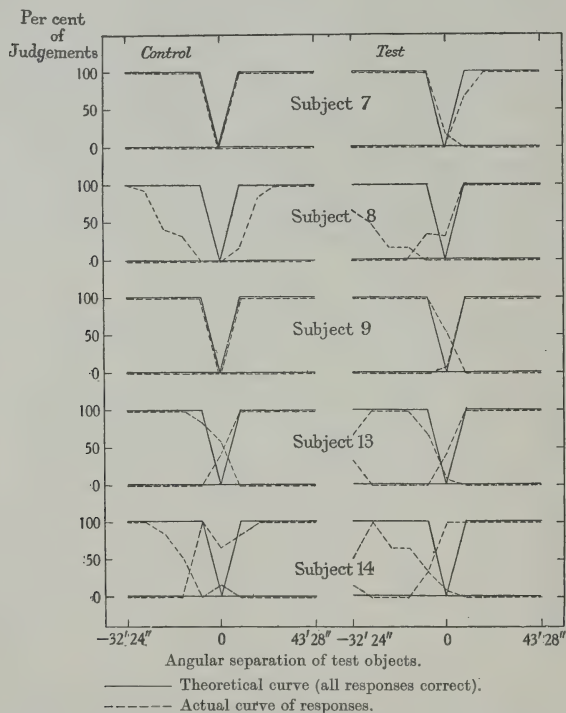
nearer the observer than the standard, and greater when it was further (it was always greater than the standard). Thus the size of the retinal image remained approximately constant at all distances, and the subject might have expected that distance was also approximately constant. But from the other sensory and perceptual data its distance would have appeared to vary, and thus a confusion be introduced. In Table IV are given the percentages of judgements of 'nearer' and 'farther' for the control and test experiments, and the same results are plotted in Fig. 2, where they are compared with theoretical curves which would have been produced if all responses had been correct. It appears that all subjects except Subject 14 were somewhat more inaccurate in their responses in the test experiment than in the control, but that the effect was not very great. Thus clearly some confusion was introduced by the conflicting data, but not sufficient to prevent the subject from perceiving that objects were not equidistant.

Table IV

Subject	Series	Judgement	Percentage of judgements with distances apart of standard and variable parallactic angles of											
			-32° 24'	-26° 36'	-20° 24'	-14° 0'	-6° 16'	0	7° 36'	15° 44'	24° 24'	33° 40'	43° 28'	
8	Control	Nearer	0	0	0	0	0	0	17	83	100	100	100	
		Farther	100	92	42	33	0	0	0	0	0	0	0	
	Test	Nearer	0	17	0	0	33	31	100	100	100	100	100	
Farther		67	50	17	17	0	0	0	0	0	0	0		
9	Control	Nearer	0	0	0	0	0	0	100	100	100	100	100	
		Farther	100	100	100	100	100	0	0	0	0	0	0	
	Test	Nearer	0	0	0	0	0	8	100	100	100	100	100	
Farther		100	100	100	100	100	54	0	0	0	0	0		
7	Control	Nearer	0	0	0	0	0	0	100	100	100	100	100	
		Farther	100	100	100	100	100	0	0	0	0	0	0	
	Test	Nearer	0	0	0	0	0	0	67	100	100	100	100	
Farther		100	100	100	100	100	8	0	0	0	0	0		
13	Control	Nearer	0	0	0	0	0	42	100	100	100	100	100	
		Farther	100	100	100	100	83	58	0	0	0	0	0	
	Test	Nearer	33	0	0	0	0	42	100	100	100	100	100	
Farther		67	100	100	100	67	8	0	0	0	0	0		
14	Control	Nearer	0	0	0	0	100	67	83	100	100	67	50	
		Farther	100	100	83	50	0	17	0	0	0	0	33	
	Test	Nearer	17	0	0	0	33	100	100	100	100	100	100	
Farther		50	100	67	67	33	8	0	0	0	0	0		

In the test experiment were also included a number of observations in which standard and variable were equidistant, while the variable varied in size. Here variation in retinal size might have been seen as variation of distance, but no tendency was apparent to estimate the smaller variables as relatively farther than the standard and the larger as nearer. Thus we must conclude again that the distance of the test object was deduced not from the relationship of apparent to retinal size, but from the other sensory and perceptual data operative. This is perhaps not surprising, since there was a pause after each observation, and thus only quite vague impressions of the apparent size of the variable may have

been retained from one observation to the next. These results are therefore not really comparable with those of Calvarezo, since no continuously moving change of retinal size was exhibited. They do, however, indicate that even under relatively unfavourable conditions, and in conflict with



The curves for 'farther' begin at 100 per cent on the left and fall to 0 per cent on the right; those for nearer begin at 100 per cent on the right and fall to 0 per cent on the left.

Fig. 2.

other data, the estimation of distance is distinctly affected by the relationship of apparent size to retinal size and distance.

But the immediacy of the perceived relation between size and distance is qualified whenever a variation in retinal size is not directly and immediately perceived as constancy of apparent size with change of

distance. This occurs in the estimation of the absolute distances of objects of unknown 'real' size, and in the estimation of relative distance when the relationship of 'real' sizes is unknown. With objects of like form there is a tendency, as in Calvarezo's experiment, to assume equality of 'real' size and therefore to perceive equality of apparent size and difference of distance. Pouillard⁽³³⁾ found that in the binocular but non-disparate perception, in a stereoscope, of two figures of identical form but different magnitude, the larger was seen as nearer in 90 per cent of cases. In series B of these experiments also it was frequently assumed by the subjects that the two test objects were of equal size (this was in fact the case). But the differences of size of the retinal images of the two objects, and the change in size of the moving object, were both very small; thus the related changes of distance were too small to be readily perceived. Possibly also certain subjects may have perceived differences of retinal size as being due to differences of 'real' size. Others, however, who were experienced in this type of experiment, and assumed 'real' size to be constant, did perceive the differences and changes of apparent distance corresponding to the differences and changes of retinal size. But it is possible that this process was not a direct and immediate perceptual one such as those described above. It may be that the subject was 'set' to observe changes and differences in the sizes of the retinal images, and perceived them as such; apparent size was no longer even approximately constant, but was observed to vary with distance. The subject then deduced, by secondary processes of inference and judgement, the corresponding changes and differences of distance. Immediate perception was made difficult by the restricted experimental conditions; but this fact in itself compelled the subject to make secondary judgements. Thus apparent size comparison was always used more in monocular than in binocular vision, and also when the surroundings were concealed from view. In the second monocular series (D) it was so pervasive that it was necessary to instruct the subject not to equate the distances by equating the sizes of the two black dots since they were of unequal size. And even then he not infrequently worked out for himself the ratio of the apparent sizes of the dots at what he thought to be equal distances of the objects, and thereafter used this size ratio for equating distances. We cannot, however, assume that there is any close relationship between the processes underlying this deliberate comparison and those which are based upon the immediate impressions of change of distance with change of retinal size when apparent size remains constant. But we shall later bring forward evidence to show that a general scheme of size relationships

of objects and the contiguous parts of their surroundings may be a factor of importance in ordinary binocular distance perception.

(3) *Experiments on clearness and brightness.*

The factors of apparent clearness and brightness seem to function in a manner somewhat similar to that of apparent size. It is important to distinguish at the outset between the situations in which distance perception is related to actual variations of brightness and clear definition at the retina, and those in which these changes are only apparent. For instance, it is well known that the factor of 'aerial perspective' plays an important part in creating the appearance of great distance. Aerial perspective is produced by the actual dimming of the retinal images of distant objects by absorption of light, diffraction, etc., in the intervening atmosphere. The effect is thus a function of the clearness or haziness of the air, and any accurate judgement of distance of such objects must thus depend on familiarity with these. According to Krauss⁽²³⁾ the phenomenon may be simulated under experimental conditions; illuminated spaces may be made to appear to shrink and expand by continuously changing the intensity of the general illumination.

Impressions of distance are also produced by the variation in the amount of light reflected from objects as their distance from the source of light varies. This phenomenon is of course particularly obvious indoors, where there is a constant decrease in amount of light reflected, and hence in retinal brightness, with increase of distance from windows or artificial lighting sources. To perceive this effect, it is necessary for the observer to distinguish between the relative whiteness or blackness of objects on the neutral (white-black) colour scale, which is largely a function of their albedo, and their brightness, which is a function of their general illumination. In ordinary circumstances this distinction is made with such accuracy as to produce the phenomenon of 'whiteness constancy'; that is to say, a decrease of retinal brightness due to increase of distance from the source of illumination is seen almost entirely as due to that increase of distance, and hardly in any way as due to change of 'whiteness'. Similarly, shadows on the parts of an object in relief are seen as shadows produced by depth effects in the uneven surface, and not as a darkening of that part of the object. A variation in shadow effects appears as a variation in orientation of the object towards the source of light. But absolute constancy may be modified under certain conditions which have been investigated in great detail by Katz⁽²⁴⁾. The essential condition for constancy is that the spatial relationships of the field should be fully

perceived and apprehended, and that the eyes should be able to accommodate accurately upon the various parts of the field and the objects it contains. If an object is viewed through a hole in a screen (called a 'reduction' screen), placed at some distance from the observer, his eye will become accommodated upon the screen, and he will no longer fully perceive the spatial relations of the parts of the object. Its surface will then lose its surface appearance, and it will appear as a vague, ill-localized mass of colour, and the shadows upon it will appear as patches of a darker colour. 'Whiteness' will no longer be differentiated from brightness.

Part of this loss of surface appearance is due to the failure by the unaccommodated eye to discriminate the small details of surface structure which Katz has termed *micro-structure*—the graining of wood, the roughness of wool, the pile of velvet, and so on. But this perception of micro-structure and also of patterning, unevenness of colouring and edges, etc., becomes less clear as objects recede from us, even when their distance from the source of illumination is unchanged and the total amount of light reflected by them per unit area remains constant. This variation in clearness, then, provides a means of estimating and equating distance in constant illumination. It was found to be of assistance in these experiments, especially under conditions making for difficulty in distance estimation, as for instance in series E. In Table V are shown the

Table V
Constant error in distance estimation, in difference of angle
of parallax from that of standard

Subject	Mean		Standard deviation	
	White surface	Grey surface	White surface	Grey surface
4	-16' 36"	6' 20"	12' 52"	9' 26"
7	-19' 16"	- 9' 40"	14' 0"	10' 30"
8	- 2' 36"	10' 32"	24' 16"	21' 48"
9	3' 52"	-10' 28"	22' 12"	16' 42"
10	-13' 48"	- 5' 44"	15' 44"	11' 20"
11	- 2' 20"	- 3' 40"	17' 14"	21' 20"
12	1' 16"	-16' 20"	16' 42"	14' 56"

results of two sets of observations in the first of which plain white surfaces were used for the test objects, and in the second, slightly mottled grey ones. All surroundings were excluded from view, and vision was monocular. It will be seen that Subjects 7, 8, 9, 10 and 12 all showed improvement in consistency in the second set, and Subjects 4, 7, 8, 9 and 10 showed fewer large errors—15, 14, 20, 17 and 11 as against 26, 19, 25, 20 and 20—. These six subjects reported that they were able to follow the experimental instructions and equate the clearnesses of the mottled grey

pattern on the test objects, and that they found the estimation easier in consequence. The results of Subject 11 were discrepant, with regard both to standard deviation and errors; but this subject had difficulty in seeing the mottled grey pattern, and therefore could not base his judgements on its clearness. Thus although the experimental results are not completely consistent, we are on the whole justified in concluding that clearness of pattern served a useful purpose in this type of experimental setting. It would also have assisted accurate accommodation; thus a slight blurring of the retinal image produced by lack of accommodation would have been more readily perceived. This may also have helped to increase the accuracy of distance equalization with the mottled grey surfaces.

We have already mentioned (see p. 125 footnote) that changes of brightness were reported as assisting in estimating distance. But it must be strongly emphasized that in all the experiments described in this paper except those in series C and H (*q.v.*) there was no variation with varying distance of the amount of light reflected to unit area of the retina of the test objects; nor of course was their albedo, and hence their 'whiteness', varied. From the introspective evidence, it seems that there must have been a decrease with increasing distance of '*insistency*' or impressiveness of neutral colour, resembling a decrease in colour saturation, and running parallel to the decrease in clearness of micro-structure. It was purely a phenomenal effect, and disappeared if the objects were viewed through a reduction screen. The term '*insistency*' is adopted from Koffka (27), p. 113), who uses it as Titchener's translation of the German word '*Eindringlichkeit*', defined by G. E. Müller (31*a*), p. 26) as "the power with which sensory impressions attract our attention". In Katz's *The World of Colour* (24) it is translated as 'impressiveness', and is assigned certain definite properties which lie outside the scope of this discussion. We shall, however, continue to use the term '*insistency*' simply in the sense described above, without reference to any further properties or qualities postulated by Koffka or Katz.

Further examples of the employment of this factor were found in a subsidiary experiment, series I, in which the variable test object was viewed monocularly through a convex lens, in such a manner that the lens gave a magnified virtual image of the object at a distance further than its actual distance. In the control observations, monocular vision was employed, but without the lens. In the test observations, data from accommodation, apparent size, and clearness were all in conflict with each other, and at first the subjects were so bewildered that they were unable to make any consistent estimates. But Subjects 7 and 10 became more or

less consciously aware that the 'insistency' (called by them the brightness) of the variable changed in a normal manner as it advanced or receded (since the subject was allowed to move the object himself, he knew fairly well which way the object was moving). Thereafter by relying on this factor of 'insistency' they were able to obtain results which compared fairly favourably with those obtained in the control experiment¹ (see Table VI).

Table VI

Constant error in distance estimation, in difference of parallactic angle from that of standard				
Subject	Mean		Standard deviation	
	Control	Test	Control	Test
6	- 5' 4"	- 2' 56"	2' 16"	21' 8"
7	3' 32"	- 5' 0"	5' 42"	12' 42"
8	10' 36"	8' 32"	5' 58"	30' 12"
10	20' 20"	0	14' 34"	14' 0"
11	- 4' 8"	- 18' 28"	20' 36"	16' 10"

Attempts were made to determine the nature of the effect of changes in 'insistency' in two ways. In one series of experiments the albedo of the test objects was varied, and in another their physical brightness as determined by intensity of illumination. In the first (series C), binocular vision was used, and the general arrangement was the same as that used in the control experiments of the first monocular-binocular series (B). The standard test object was a piece of plain (unmottled) grey paper, pasted on cardboard, and for the variable were used a graded series of grey papers, seven in number; the middle one of the series was the same grey as the standard. The results of these experiments are given in Table VII; the constant errors of estimation of equality of distance are

Table VII

Constant error in distance estimation, in difference of parallactic angle, for Grey No.			
Subject	1 and 2	3, 4 and 5	6 and 7
6	1' 12"	1' 4"	0' 4"
7	1' 24"	0' 36"	0' 4"
8	1' 8"	0' 48"	0' 44"
10	0' 8"	-0' 4"	-0' 16"
11	1' 12"	0' 16"	0' 16"

grouped and averaged in three groups, the first and third including estimates for the two lightest and the two darkest greys respectively,

¹ Subject 8 also employed the factor of 'insistency', but he tried to co-ordinate this with data from accommodation sensations, which may have accounted for the inaccuracy of his results.

the second, those for the three medium greys. Although the number of observations was too small¹ for calculating standard deviations, or for giving very reliable results, it will be seen that every subject showed a tendency for the positive constant error (over-estimation of distance of variable) to decrease from the lightest to the darkest greys. Thus there was a slight tendency to set the lighter greys nearer than the standard and the darker ones further away, which means that the lighter greys appeared relatively farther away than the darker ones—in spite of the fact that the subjects noticed that surfaces of different greys were used, and considered that their estimation was unaffected by this difference. Thus the effect produced must have occurred as an immediate perception unaffected by any secondary judgement process. The subjects reported

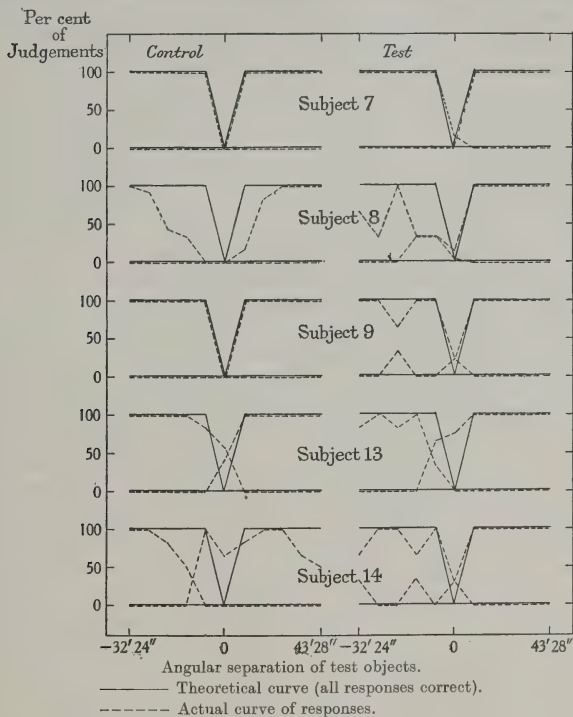
Table VIII

Subject	Series	Judgement	Percentage of judgements with distances apart of standard and variable parallactic angles of											
			-32° 24'	-26° 36'	-20° 24'	-14° 0'	-6° 16'	0	7° 36'	15° 44'	24° 24'	33° 40'	43° 28'	
8	Control	Nearer	0	0	0	0	0	0	17	83	100	100	100	
		Farther	100	92	42	33	0	0	0	0	0	0	0	
	Test	Nearer	0	0	0	33	33	15	100	100	100	100	100	
		Farther	67	33	100	33	33	4	0	0	0	0	0	
9	Control	Nearer	0	0	0	0	0	0	100	100	100	100	100	
		Farther	100	100	100	100	100	0	0	0	0	0	0	
	Test	Nearer	0	0	33	0	0	23	100	100	100	100	100	
		Farther	100	100	67	100	100	23	0	0	0	0	0	
7	Control	Nearer	0	0	0	0	0	0	100	100	100	100	100	
		Farther	100	100	100	100	100	0	0	0	0	0	0	
	Test	Nearer	0	0	0	0	0	0	100	100	100	100	100	
		Farther	100	100	100	100	100	17	0	0	0	0	0	
13	Control	Nearer	0	0	0	0	0	42	100	100	100	100	100	
		Farther	100	100	100	100	83	58	0	0	0	0	0	
	Test	Nearer	0	0	7	0	67	77	100	100	100	100	100	
		Farther	83	100	83	100	33	0	0	0	0	0	0	
14	Control	Nearer	0	0	0	0	100	67	83	100	100	67	50	
		Farther	100	100	83	50	0	17	0	0	0	0	33	
	Test	Nearer	33	0	0	33	0	31	100	100	100	100	100	
		Farther	67	100	100	67	100	31	0	0	0	0	0	

that they were somewhat confused by the changes which they observed in neutral colour quality of the variable, the more so as the particular grey papers used seemed to show a slight chromatic variation also. It is clear, however, that the differences must have been perceived in part as differences of 'insistency', and that these differences were equivalent to those normally produced by changing the distance of the medium surface. Apparently the lighter grey surfaces were less 'insistent' than the darker, and the degree of 'insistency' was in the nature of degree of saturation or of clearness of neutral grey colour.

¹ The experiments were discontinued because of the confusion which was introduced by the apparent differences of chromatic hue between the grey papers. Hence also the albedos were never quantitatively determined.

The effect of changes of physical brightness produced by changes of illumination was studied in binocular vision in a series of experiments (series H) similar to those in which size variation was introduced. Variable and standard objects were kept stationary and exposed at different distances apart. The physical brightness of the variable was kept constant



See note to Fig. 2.

Fig. 3.

in the control series; and in the test series it was slightly decreased when the variable was nearer to the subject than the standard, increased when it was farther. The results are shown in Table VIII and plotted in Fig. 3. It will be seen that a certain confusion was introduced into the estimation of distance by the objective variation in brightness; this is most noticeable in the case of Subject 9, who was not much affected by size variation.

In cases where the two objects were equidistant, and the brightness of the variable was varied, there was no tendency for the darker variables to appear farther away than the standard, and the brighter ones to appear nearer. This then seems to show that the change of 'insistency' observed with changed distance is not in the nature of a change of physical brightness—that farther objects do not appear less 'bright' (provided that illumination is constant) but less 'insistent'. Apparently, however, real changes in physical brightness may have a confusing effect on change of 'insistency' with distance.

The experiments on brightness which we have described cannot be considered to do more than give the barest indication as to the relationship between distance perception and relations of clearness, brightness, 'whiteness' and 'insistency'. The physical factors were insufficiently controlled and the observations too few in number to warrant any final conclusions. Moreover, Henneman⁽¹⁶⁾ has recently shown that these effects are closely related to the nature of the surrounding field—its size, homogeneity and brightness—all of which would also have to be taken into consideration in forming such conclusions. But these experiments do support the view that the related qualities of phenomenal clearness and 'insistency' of objects may be important factors in producing distance effects, not only under the conditions of these experiments, but also in normal distance perception. For the experiments in series C seemed to show that there was an immediate tendency, of which the subject was unaware, to see the decreasing 'insistency' of the surface as an increase of distance. This effect must, however, be clearly distinguished from the changes of brightness associated with 'whiteness' constancy which occur when there is change of distance from the source of illumination. This phenomenon, which we described on p. 134, is in most ways analogous to that of size constancy. But it was not studied in these experiments, which were confined to the investigation of the phenomena of clearness and 'insistency'.

(4) *Further conclusions as to the function of disparity.*

It is convenient at this point to draw attention to certain cases in series G and H in which, when the two objects were only a short distance apart, their spatial relationship was correctly reported, but when their distance apart was increased, the subject was uncertain as to their relationship. He could see that the two objects were not equidistant, but not which was the nearer and which the farther. These cases all occurred when the variable was farther away than the standard, and when the

conditions were complicated by brightness or size variation, particularly the former. Thus apparently the time of exposure was too short to allow a thorough appreciation of the direction of disparity between objects separated at some distance from each other. Some subjects definitely reported that they found it difficult to get the variable into focus, that is to say, to perceive the degree of disparity and hence converge and accommodate on it correctly. Others reported that the variable seemed to change its position during the exposure, which indicates that convergence and accommodation fluctuated uncertainly. Now in all previous experiments, the difference of disparity, convergence and accommodation between the two objects had gradually and continuously decreased, and the subject was not required to make his estimate until the difference had almost disappeared. Here, however, he was obliged to estimate a considerable degree of disparity in circumstances of some conflict and ambiguity; he could not confirm his estimate by repeating his observations several times, since the time of exposure was limited. Neither had he time to fit the perceived disparity into a continuous series or scheme of degrees of disparity adapted to successive parts of the receding spatial surroundings. Consequently he could perceive that the two objects provided disparate retinal impressions, that they were not equidistant from him; but this disparity did not in itself indicate whether one object was nearer or farther than the other. These observations, then, afford a convincing proof that disparity sensations can in general supply only an immediate 'all-or-none' type of information; objects are seen as equidistant or non-equidistant. In normal distance perception, a sequence of slightly differing degrees of disparity can be formed in close inter-relation with the perception of a sequence of objects also graded with regard to retinal size, 'insistency', etc. When, however, this visual setting is absent, or when there is insufficient time for the correlation of the various visual sequences, the subject has some difficulty in determining from disparity alone whether the variable is nearer or farther than the standard. This difficulty is apparently increased when the amount of disparity between variable and standard is increased.

(5) *Experiments on the effect of surroundings.*

We have indicated from time to time during this discussion that we regard the general spatial setting including the whole visual field (and probably also the proprioceptive field) as the essential factor in normal spatial perception, co-ordinating and integrating all the other perceptual data. There is much previous experimental evidence to show the im-

portance of the perception of surroundings to the estimation of distance. Thus Bourdon⁽⁵⁾ and Jastrow⁽²¹⁾ found that it was impossible, with the head held stationary (to eliminate sensation of parallax movement), to localize a bright spot of light in a completely dark room of unknown dimensions, either absolutely or relatively to another spot of light, even in binocular vision. But if only a slight gleam of light was introduced giving a vague idea as to the nature of the surroundings, localization became possible. An experiment by Jaensch^(20a) showed that relative localization might also be considerably affected under more favourable conditions. If an observer was presented with three vertical threads, the middle one of which was nearer to him than the others, he perceived a wedge-shaped structure protruding towards him. If the threads were replaced by luminous wires in a dark room, the wedge was greatly reduced and might disappear altogether, the wires appearing to lie in one plane. Thus when disparity sensations were not at all clear, a complete absence of perception of surroundings made perception of distance impossible. Again in the prism series it was noted that confusion occurred as the result of the distortion of the spatial setting which in most cases caused the subject to ignore all perceptual data except the one set clearly emphasized by the experimental conditions. In the monocular series, the difficulties of estimation were greatly enhanced when the surroundings were concealed from view, or when there was a wide spatial separation between the test objects and the surroundings. When even a partial view of the surroundings was allowed, provided that this spatial separation was not too great, an increase of accuracy and consistency in estimation resulted. This is demonstrated by the results of series F given in Table IX. Monocular vision was used and the plain white test objects

Table IX

Constant error in distance estimation, in difference of angle
of parallax from that of standard

Subject	Mean		Standard deviation	
	Control	Test	Control	Test
4	-16' 36"	-11' 32"	12' 52"	9' 26"
7	-19' 16"	-12' 36"	14' 0"	7' 52"
8	-2' 36"	-3' 32"	24' 16"	18' 48"
9	3' 52"	-17' 12"	22' 12"	13' 56"
10	-13' 48"	3' 40"	15' 44"	9' 44"
11	-2' 20"	-6' 32"	17' 14"	13' 26"
12	1' 16"	11' 20"	16' 42"	13' 58"

with a black dot upon each. In the control observations, no surroundings were visible; in the test series, the subject could see part of the side walls

of the box enclosing the apparatus. There was no contact between the test objects and these sides, but the distance between them was only 6 in. He could also see part of the back wall beneath the upper and above the lower test object. The consistency of the results was in every case greater in the test experiment than in the control. Indeed, for Subjects 4, 7, 9 and 12 the consistency was appreciably greater when the surroundings were visible in monocular vision than when they were concealed in binocular vision (cf. Tables III and IX), since, as we pointed out on p. 127, it is more difficult to estimate disparity correctly when the surroundings are completely concealed. But some of the observers themselves commented that it was difficult to gain much assistance from the relationship to the surroundings because (1) no points of contact between test objects and surroundings were visible, and (2) the actual size of the spaces between the test objects and the side walls was unknown, and also the ratios of these sizes for the two test objects; that is to say, the subject could not estimate the absolute or relative distances of the objects by comparing the sizes of the retinal images of these spaces, which of course varied continuously in perspective as the distances of the objects varied. This then gives us a further indication as to how size relationships may be utilized when the information given by other perceptual data is inadequate as a basis for distance estimation. The observer reacts not only to the changing retinal size of the test objects, but also to the retinal sizes of the various contiguous parts of the surroundings as they recede from him; and these deliberately observed size relationships give additional information beyond what can be supplied by the size relationships of the test objects alone.

Again we noted in series G and H that if a speedy estimate of relative distance was required, sensations of disparity might be inadequate if the test objects were far apart. This inadequacy apparently arose because the test objects were not sufficiently close to the surrounding walls to form with them a spatially continuous integrated pattern; thus the observer could not perceive a gradually decreasing or increasing series of degrees of disparity by passing his eyes gradually backwards or forwards from one object to the other *via* the surrounding planes; but was compelled to estimate immediately and, as it were, in the void, the disparity between the two objects without observing any intermediate degrees of disparity.

III. DISCUSSION AND CONCLUSIONS

These observations all point to the paramount importance of a close integration of perceptual pattern including the test objects and the receding planes of the surroundings; this integration, it may be supposed, is produced not only by contact between the test objects and the surrounding planes, but also by a phenomenal continuity of configuration. That is to say, in so far as the test objects can be structurally related to any three-dimensional configurational pattern existing in the immediate surroundings, to that extent can they be readily and accurately localized with regard to those surroundings. Moreover, it seems probable that the greater the 'goodness' of the configuration, and the greater the coherence of its parts, the easier and better the localization. Kopferman⁽²⁸⁾ indeed has shown that the separate parts of a stimulus constellation, such as the Necker cube which normally appears three-dimensional, will if drawn on separate glass slides situated behind one another at different distances from the subject, fuse together to form the integrated three-dimensional structure of the Necker cube. Thus the planes in which the various parts are perceived to lie are the continuously and gradually receding planes of the Necker cube, and not the segregated planes in which the glass slides are actually situated. The data provided by the structure of the configuration are more important than those provided by disparity, accommodation, etc.

Thus it is very probable that in normal perception, when there is no conflict between the data, such a configurational structure may be extended to include, not only the relative and absolute localization of all parts of the general spatial setting, but also our whole phenomenal experience of space, and of three-dimensional recession and perspective. The three-dimensional configurational structure and patterning of spatial extents depend for their effect on their inclusiveness and continuity from near to far distant objects. Moreover, the 'goodness' and continuity of the configurational structure is greatly enhanced by the existence of perspective effects, overlapping of planes, shadows thrown by certain parts of the field on other parts, etc. Integrated with this structure are gradually and continuously changing series of all the relevant perceptual data—decreasing degrees of disparity, convergent and accommodatory tensions, retinal size, clearness, 'insistency' and brightness. The impression of space and distance is thus most adequate and convincing when the continuity of these series is unbroken and is clearly perceptible; but it is impaired if in any way the nature and direction of change in one

series of data is concealed or conflicts with those in the others. It is probable that in normal spatial perception the data are not separately observed and assessed as segregated entities, to be matched and checked against each other at the highest cognitive levels, but that their changing inter-relationship is perceived directly and as a whole. Thus our normal experience of space is made up of these graded perceptual series all closely integrated and interdependent, co-ordinated within a general scheme which is based upon:

(1) The visual perception of the configurational arrangement of all the immediately perceptible parts of the environment—what we have called the general spatial setting—including the perception of our bodies in relation to that environment);

(2) Perception of orientation to that spatial environment; this is visual, labyrinthine and proprioceptive;

(3) The inter-relationships of these immediate percepts with all previous experience of space and space relationships, however remembered. (There is no doubt that such relationships are remembered, but it is not advisable here to enter into any theory as to the nature of that remembering.)

Any consideration of the nature of (2) and (3) is beyond the scope of this paper. But we have attempted to show the essential nature of the relationship of the various sets of perceptual data described in this paper to the percept described in (1). Whether all the data are absolutely essential to the perception and estimation of distance is impossible to judge. We have shown that impairment of any one of them is liable to affect adversely the processes of perception and estimation, but in general the relationship to the surroundings and general setting has also been affected. It is possible that if the latter were retained unimpaired, the observer might be able to localize satisfactorily objects for which certain of these perceptual data were lacking, provided that no conflict was involved with other data. This is a problem to which it is hoped to proceed in future.

It may be urged in contradiction to the theory of the essential nature of the configurational setting that we can in fact segregate certain of these perceptual data and employ them as a basis for the estimation of distance, apart from their relationship to each other in the spatial setting. But clearly in most experimental settings and in particular in all the experiments described here, the subject was well aware of the broad spatial relationships of the setting of the test objects; he knew, for instance, the maximum distances to which they could recede, and usually had some

idea as to their actual size. Bourdon's experiment⁽⁵⁾ on the localization of bright points of light in a totally dark room, the spatial structure of which was unknown to the subject, seemed to show that in the absence of all knowledge of spatial setting both absolute and relative localization in binocular vision were impossible. Wundt's experiment⁽⁴¹⁾ also showed that absolute localization of a black thread seen through a tube against a white background was very uncertain even in binocular vision, though according to Bourdon⁽⁵⁾ it was assisted by such view of the surroundings as he could obtain. Thus estimation of distance must depend upon the subject's ability to fit the object into the spatial setting, perceived or inferred. Absolute distance estimation cannot proceed from a concept or idea formed in the void, or even in imaginal space if this is divorced from all relationship with perceived space in a general scheme such as that described above. The closer the relationship to perceived space and the more exact the knowledge, not only of its gross structure but also of the small details thereof, the greater the accuracy of localization.

But does relative localization also necessarily involve fitting the objects compared into a spatial setting? Bourdon's experiment⁽⁵⁾ seems to give an affirmative answer. It is clear, though, that the perception of the disparity between two points of light must have been very difficult if not impossible. Other observations show that distance comparison can be carried out, sometimes in terms of 'nearer' and 'farther', sometimes only in terms of 'equidistant' and 'non-equidistant', under certain specialized conditions:

(1) By means of sensations of disparity, provided that sufficient time is given for it to be accurately assessed—particularly if the degree of disparity is large. Equalization of distance is easier than comparison in terms of 'nearer' and 'farther'.

(2) By means of apparent size, brightness, 'insistency' and clearness, provided that the subject has some knowledge of the 'real' size, brightness and clearness of the objects.

But what relationship do the estimates formed in these ways bear to those which are made when the objects can be easily fitted into the spatial setting? We have shown that they are objectively less accurate and much less consistent, and that subjectively they are made slowly and with difficulty, and that the observer has no certainty as to their correctness.¹ Except for immediate estimates based on disparity, many of them are

¹ Except under very special conditions, as for instance in certain stereoscopic instruments, when the angular separation of the two objects is very small; estimates can then be made with great accuracy, but as a rule slowly and with difficulty.

in the nature of secondary judgements rather than of perceived relationships of distance; all the relevant data are taken into account, deliberately balanced against each other, some accepted and others rejected, and the final judgement based upon the former. In any case, we must reluctantly conclude that we know very little as to the bearing of such processes upon the processes of perception and estimation employed in normal and ordinary circumstances. There is a gradual change from one to the other as the observer gradually becomes more aware of the exact relationship of the position of the objects to the structure of the spatial setting. And as it is impossible in practice to cut out all knowledge of this structure without directly affecting the perceptual data described above, we cannot sharply distinguish in introspection the processes which occur with and without this knowledge.

But it does appear that if we wish to study the normal processes of distance perception and estimation, we must work with some kind of general spatial setting with which the various sets of perceptual data are more or less completely and satisfactorily integrated. We have suggested that a vitally important factor is the structure of the setting, involving continuity of change in all, or as many as possible of, the relevant perceptual data; and in subsequent experiments we hope to deal more fully with this factor.

IV. SUMMARY

1. An attempt has been made to distinguish between the primary and immediate perception and estimation of depth and distance, and secondary judgement relating to distance; and so to arrange the experimental situation, making it resemble as nearly as possible the conditions of normal distance perception, as to allow the primary perceptual processes to occur, as well as secondary judgements.

2. This was found to be difficult, since a closer study of any of the conditions of occurrence of distance perception necessitated the elimination or falsification of some of the more important perceptual data; and this was likely to interfere with primary perception, replacing it by secondary judgements, and giving an artificial prominence to certain of the remaining data, especially of those obtained from the comparison of size of retinal images.

3. Distances could be compared and equalized by means of sensations of disparity, under certain quite specialized conditions. The functions of accommodation, apparent size, clearness, 'insistency' and brightness were also examined and discussed.

4. It was concluded that the most essential feature of the processes of distance perception and estimation was the visual perception of a three-dimensional configurational structure with which were closely correlated the perceived perspective form of the general setting and surroundings (and the inter-relationships of its constituent parts), and the graduated series of impressions of disparity, size, clearness, 'insistency' and brightness.

In conclusion, my acknowledgements are due to the Medical Research Council for a personal grant which made this work possible. I wish to thank Prof. Bartlett for his advice and assistance, and for permission to work in the Cambridge Psychological Laboratory; Mr R. W. Cheshire, of the Admiralty Research Laboratory, for much useful information and discussion; and all those who have helped me with advice and by acting as the subjects of these experiments.

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CONSCIOUS ACTIVITY IN CO-ORDINATION OF REPETITIVE MENTAL WORK AND RHYTHMIC TIMING

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I. DESCRIPTION OF THE EXPERIMENT

It has been shown that neither pattern nor rate of auditory rhythmic timing accompanying work has an effect upon rate of performance in a psychomotor repetitive task unless the worker consciously intends to effect such a co-ordination. In the experiments referred to, there is no unconscious general influence of rhythmic timing upon working rate, i.e. raising or lowering it as the rate of the rhythm is increased or decreased. Any effect that objective timing may have upon the rate of performance is through conscious intent, but the development of an intent to co-ordinate rates of work and objective timing was not usual for a worker. Awareness of an auditory rhythmic accompaniment was reported as extraneous to the conscious working process (2,3).

With this knowledge as a basis, an experiment¹ was planned to study the conscious activity of workers during intentionally co-ordinated repetitive mental work and rhythmic timing. The experiment consisted of the written performance of simple additions with instructions to co-ordinate with two accented beats of a pleasing four-note auditory rhythmic unit. The general arrangement of the apparatus for the administration of the auditory rhythmic unit and the recording on a kymograph record of its timing are shown in the previous paper (⁽³⁾, Plate I and Fig. 1, p. 145).

(a) *The rhythmic unit.* The rhythmic unit consisted of the following four tones in order: low G (783 d.v.); B (987 d.v.); D (1174 d.v.); high G (3135 d.v.), which were equally timed and followed by a rest which was equal in time to one-fifth of the unit. In introspective report, the usual description was of a double-accented rhythm, beginning with a long heavy accented note (the low G), followed by two unaccented and connecting tones, and ending with a shorter and more lightly accented note (the high G).

(b) *The instructions.* Instructions were administered, as follows:

Allow an outside force to set the pace for you. You be the machine and let the outside force drive you. This outside force will be the auditory accompaniment, in which two accents per unit of rhythm will be easily recognized. Direct your awareness upon the auditory accompaniment and beat out the accents by adding one problem per accent (two problems per unit of rhythm). Let the pace of your work be determined entirely from outside.

(c) *The task.* The task consisted of the author's *Speed Addition Test*,² in which the problems are a chance distribution of totals of less than nine, arranged in rows of thirty. This offered a highly regular repetitive task in which the items to be co-ordinated with the rhythmic timing would be as regularly equal in difficulty as any known industrial or educational task. A test consisted of 1 min. of performance of the task, and a day's performance consisted of approximately ten tests, which with the time consumed for introspections required usually a little more than 1 hour.

(d) *Introspection.* Technical emphasis was placed upon securing introspective data, as this was to be a study of conscious activity in a psychomotor repetitive task co-ordination with objective rhythmic timing. Descriptive introspections were requested following each 1 min. test, which were introduced with the words: "Tell me everything that happened in that test concerning what you did."

¹ This investigation was performed in the Psychological Laboratory of the University of Cambridge, England, in 1932.

² Published by the Department of Psychology, New York University.

II. A PRELIMINARY EXPERIMENT

Nine observers trained in previous experiments were selected for a preliminary experiment to test initial ability in the performance of the task when timed by the double-accented rhythm. In this preliminary experiment, the double-accented rhythm was followed perfectly by six of the nine observers at a comfortable pace determined from previous individual records. One more observer showed relatively greater difficulty in working in time with the rhythm. The remaining two observers failed completely in co-ordinating their performance with the objective timing. Thus, for this small group, 78 per cent had some initial ability in co-ordinating performance in the task with the objective timing when working at a comfortable rate.

III. THE MAJOR EXPERIMENT

Four of the nine observers (observers E, F, G, and H)¹ were selected for a detailed analysis of the conscious activity by which repetitive mental work at various rates is co-ordinated with objective timing of a rhythmic nature. Three of the observers (observers E, G, and H) were selected from those who co-ordinated performance and rhythmic timing exactly at comfortable rates, and one observer (observer F), who had difficulty in co-ordinating performance with the timing, was included for the added information his study might offer. Particularly, we wanted to know what will happen in conscious activity when the worker is working at his highest rate of performance, as previously determined, and if he can work more efficiently and with less conscious guidance at these higher rates where there is objective timing. Also, it was thought possible that objective timing might be a cue for increasing performance.

Table I

Observer	Number of 1 min. tests of previous practice	Highest previous rate in problems per minute	Increase (%)
E	21	132	—
F	31	116	37.6
G	44	129	30.4
H	42	153	38.7

(a) *Previous practice in task.* Amount of previous practice in the task with intent to reach the highest rate possible, and without co-ordination with rhythmic timing, is shown in Table I for these four observers in

¹ Acknowledgement is made of the assistance of Prof. F. C. Bartlett, Drs J. M. Blackburn and P. E. Vernon, and Miss M. D. Vernon, of the Cambridge Laboratory, who were the observers in this experiment.

number of minute tests, highest rate achieved in problems per minute, and percentage increase of last over the second test of the series.

Observer E had 719 min. of previous practice of which only 21 min. were with racing intent, and as no initial performance was taken with racing instructions the percentage increase cannot be computed.

(b) *Performance results.* The fastest previous self-directed performance is used as the standard rate in the performance co-ordinated with the rhythmic timing. In Table II the amount of error¹ in problems performed, when compared with the timing rate, is indicated in plus and in minus quantities for observers E, F, G, and H in the observers' columns at the right. The scale of timing rates (col. 1) shows the standard rate for each individual as zero, which is each observer's previous highest rate in self-directed performance in the task. The timing rates above and below this standard rate are shown with plus and minus signs, and are given as the number of accents per minute of the rhythmic timing.

A wide range of rhythmic rates was used for each observer, as will be noted in Table II. The observers' columns in Table II show losses primarily, rather than gains. The percentage of tests in which performance was accurately co-ordinated with the timing above and below the standard rate for each observer is shown at the bottom of the table, with the number of tests in parenthesis.

The results in Table II indicate considerable variability between these observers in ability to co-ordinate performance with the rhythmic timing, as is shown by the percentage of the tests in which the performance equalled the timing rate. It will be noted for observer F that there is about the same number of tests with exact and inexact co-ordination at the various rates administered. There is a slightly greater proportion of tests in which co-ordination of performance with the rhythmic timing is exact at the higher rates, i.e. from -12 to -22 (62 per cent), than there is at the lower rates, i.e. -22 to -32 (54 per cent). Observer F is the one in the preliminary experiment co-ordinating work and timing with difficulty at a comfortable pace, and it is suggested here that it is not a certain rate that is difficult but the co-ordination itself.

Observer E, on the other hand, co-ordinated accurately to ten problems per min. below his standard rate, and in all rates above there is

¹ The amount of error is the difference in plus and minus quantities between the number of problems performed and the number of accents (two per rhythmic unit) of the rhythmic timing. This measure does not indicate the accuracy with which each problem of the task was co-ordinated with each accent of the objective timing, but only the total gains and losses in the co-ordination.

Table II. *Co-ordination of performance with timing rate in relation to standard rate of observers (highest previous rate)*

Timing rate with zero as standard performance rate for observers	Amount of error in performance by observers			
	E	F	G	H
-32	0	-2, +2	—	—
-31	—	—	0	0, 0
-30	—	-2, 0	—	—
-29	—	—	-4, 0, +2, 0	0, 0
-28	—	0, 0	—	—
-27	—	—	0	0
-26	0	-4, -2	—	—
-25	—	—	0	—
-24	—	0, 0, 0	—	—
-23	—	—	0	0
-22	0	-2, 0, -2, 0	—	—
-21	—	—	-2, 0, 0	—
-20	—	0, 0, 0, -9	—	—
-19	—	—	—	—
-18	—	-10, -10, -3, -2, 0, 0	—	—
-17	—	—	0	0
-16	0	0	—	—
-15	—	—	0, 0	—
-14	—	-15	—	—
-13	—	—	—	0
-12	—	0	—	—
-11	—	—	0	—
-10	0	—	—	—
-9	—	—	—	0
-8	-5	—	—	—
-7	—	—	0, 0	—
-6	-3	—	—	—
-5	—	—	—	0
-4	—	—	—	—
-3	—	—	0	—
-2	-2	—	—	—
-1	—	—	0, +2	—
0	-9, -1	—	—	—
+1	—	—	—	0
+2	—	—	—	—
+3	—	—	-2, 0	—
+4	—	—	—	—
+5	—	—	—	0
+6	-5, -7	—	—	—
+7	—	—	—	—
+8	—	—	—	—
+9	—	—	-10	—
+10	—	—	—	—
Percentage of tests equalling timing rate, and below or equal to observer's standard rate	63 (10)	54 (28)	80 (20)	100 (10)
Percentage of tests equalling timing rate, and above ob- server's standard rate	0 (2)	—	33 (3)	100 (2)

inaccurate co-ordination. Both observers G and H accurately (observer G usually) co-ordinated with the rhythmic timing at all rates below their standards (highest previous performance). Also, both exceed this rate in co-ordination, with observer G co-ordinating with the rhythmic timing at three problems per min. above his standard of 129 per min. and with observer H doing so at five problems per min. above his standard of 153. Tests of the highest limit where observer H might have co-ordinated performance with the rhythmic timing were not administered.

IV. CONSCIOUS ACTIVITY OF OBSERVERS

A study of the descriptive reports of conscious activity, which followed each test, shows how the co-ordination between performance and the rhythmic timing was effected in conscious stimulation.

(a) *Observer E.* Observer E, one of the three efficient workers of the preliminary experiment, performed the task perfectly, with conscious ease, and with irrelevant thinking at low rates in relation to his standard rate (highest previous record under self-direction). The tests administered to this observer were given in an irregular ascending series of rates during 1 day's observation.

He reports in tests at low rates: "Kept time with each rhythmic beat. Got behind on two occasions and verbalized, 'you had better pay attention'. When behind, feelings of tension and kinaesthesia in arm muscles and when in time, relaxation. There is occasional irrelevant thinking. My system is to look at number ahead. I have a mixture of kinaesthetic and auditory imagery concerning next sum. I don't feel driven and the only feeling of push is when I get behind." He was able to co-ordinate in this manner at timing rates ten beats below his standard, after which he reports he got behind because irrelevant thoughts not connected with the task became focal. He says: "When I get behind there is worry and increased tension in the whole arm. It is unpleasant to write out of beat." When working at a rate of six problems per minute below his standard, there is a loss of three beats, and the report: "When behind I completely lost the accent. Must have written them out of beat and it is difficult to get back without missing one." 'Catching-up', which he did at low rates, becomes impossible and the observer pauses to regain co-ordination with the rhythmic timing. At a rate two beats per minute below his previous highest record, he reports: "When I am hurrying it is all right. It is after I have been hurrying and realize I have caught up that I miss a beat. I thought, 'It is about time for a breakdown', and I had one. I do my best to concentrate on the task, following my system." And again, when the timing rate is equal to his standard, he reports: "I simply can't catch up. The only thing is to miss a beat and my system smooths out. If I should go on as before, I would be completely out of timing. When out of accent my mind becomes a blank and I write down anything—wrong answers." But at a timing rate equal to his standard, he equals the timing within one problem per minute, and he reports "sensitivity of

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hurrying to catch-up, in bending forward, tension in hand and neck". By now there is objective evidence of greatly increased motor effort. In the tests with rates above the observer's highest performance under self-guidance there is the report of marked physical effort.

In the early tests at low rates the intent is to 'catch-up' with the rhythmic timing when behind. But as rates closer to the observer's standard rate are introduced, the intent changes to 'lose a beat in the timing when impossible to catch-up', but tremendous pressure is maintained to co-ordinate performance with the timing throughout all tests. The intent, it would seem, is first to perform the task accurately and, second, to co-ordinate with the rhythmic timing. The objective timing does not carry along the writing. But the co-ordination of performance and rhythmic timing is accomplished with a high degree of efficiency by this observer in relation to his previous highest performance under self-direction, as is shown in Table II.

(b) *Observer F.* It will be recalled that Observer F had demonstrated considerable difficulty in the preliminary experiment, and he was included in the major experiment to study the process of conscious co-ordination under conditions of greater difficulty, with the hope that the conscious mechanisms involved in the co-ordination might be more focal and introspectively reported. The tests administered to Observer F consisted of two irregular ascending series of rates spread over four days' observations.

Observer F's early reports indicate his intention to "keep the rhythm by beating it out with the pencil. The rhythm is an emphasis of the articulation of the sums which I carry-on in twos. I am writing one and adding the next at the same time. Then I have to wait to put it down, after which I start adding the next. The timing of the rhythm is complicated: partly in articulations, partly in writing, partly in tensions. I beat with pencil, emphasizing the stronger of the two accents (the high note)." There are few, if any, irrelevant awarenesses reported and by the fourth test this observer says, "I am beginning to feel I can catch-up if I drop behind, and it is not necessary to wait for the rhythm, as before. The rhythm is what pulls me on—to get my writing synchronized with the accents. There is articulation of the sums throughout. I feel the auditory thing pull me up and I accept the outside thing as director. I am actively looking for the accents. If swinging right, it is pleasant, if not, it is unpleasant, which hangs on for some time after I get into the swing again."

So it appears in the early tests that co-ordinating performance with the rhythmic timing is a highly focal process for Observer F, and he intentionally develops numerous cues to perform the co-ordination. The process is definitely a dual activity (1) of performing the additions and (2) of writing them in time with the rhythm.

In later tests he reports verbalizations before beginning of a test about its rate, such as, "Will it be quicker?" and an answering verbalization after the beginning of the test of "No, same speed". Focal throughout are the accents of the rhythm and the articulation of the sums. In a test when behind he reports verbalizing, "I am going to catch up". Went against rhythm until I was writing first of my two sums on the first beat of the rhythm. Then verbalized 'Have I done more or less?' Then error, and I let two beats go by, then two more, until I got in." (This test was 22 beats below the observer's standard.) Again when out of time there is the report: "Didn't get started right. Whether difference is in me or stimulus, don't know. Felt as if I was synchronizing writing, but wouldn't be surprised if it were going at a different speed. A great deal of verbalized questions." (This test was 20 beats below the observer's standard and his performance was nine below this rate.) In another test he reports, "I can't get same rhythm. There were verbalizations about it. Wasn't working to rhythm at all. I lost it. Tried to speed up, but don't think I ever dropped into the rhythm." (This test was 14 beats below the observer's standard and his performance was 15 below this.) When successfully co-ordinating with the timing, there are reports of "continuous verbalization of sums". When awareness "was too focal on rhythm" he reports, "I am likely to get out or make error". There are verbalizations during the tests to "carry on", and in a test when he got behind, he reports: "What I want to do is to get ahead of the beats. I got on the wrong accent with my twos and had to wait to get back." Frequently in later tests he reports, "if writing is going smoothly, there is nothing in awareness but the verbalization of the sums". At the highest rate of timing administered (twelve beats below the observer's standard rate) there was perfect co-ordination and it is reported: "Definitely quicker. No feeling of hurry until I drop behind and hurrying to catch up is localized in the writing movements."

While the process of co-ordination is quite focal in awareness in all tests, the degree of this awareness throughout a test seems to grow less with practice. Conscious intent to do something always develops when in difficulty, while the cues to effect the co-ordination gradually narrow down as the work progresses to the verbalizations and the writing of the sums in time with the accents of the rhythm. Also, there are a few more extraneous verbalizations about the task as practice increases.

When there is exact co-ordination of performance and the rhythmic timing, the conscious process is described as uninterrupted activity, with occasionally a struggle to catch-up where intent is developed to do so. Where exact co-ordination exists between performance and rhythmic timing, it is estimated with accuracy by the experimenter from the introspections of this observer in 88 per cent of the tests. Where there is inexact co-ordination of performance and the rhythmic timing, the conscious process is described as a breakdown of intent, and it exists either in a loss of awareness of the accents, or awareness that the writing is behind the accents and intentionally letting beats go by without writing, or in a complete loss of awareness of the rhythmic timing. Where

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inexact co-ordination exists between performance and rhythmic timing, it is estimated with accuracy by the experimenter from the introspections of this observer in 91 per cent of the tests. In all tests, co-ordination of performance and rhythmic timing is estimated accurately from the introspections in 89 per cent of the tests.

The tests administered to Observer F were in the form of two irregular advancing series of rates. The average rate of the first series was 21 below the observer's standard rate, with a range of -28 to -15 for thirteen tests. The average rate of the second series was 23 below the observer's standard rate, with a range of -32 to -12 for fifteen tests. Performance is more efficient in the second series of tests. All the tests with errors above + or -2 were in the first series. Co-ordination of performance and rhythmic timing in the first series is exact in only 31 per cent of the tests. In the last series it is exact in 73 per cent of the tests. Intent is more efficient in the second series. In the first series there is an estimated breakdown of intent in 63 per cent of the tests, while in the last series it is estimated by the experimenter as occurring in only 33 per cent of the tests. Thus, improvements in both intentional activity and performance are seen to parallel each other with practice in this work of co-ordinating the task with rhythmic timing.

(c) *Observer G.* It took four preliminary tests for Observer G to become aware of the timing rate of the rhythm, although the experiment followed a practice period in which this observer had demonstrated ability to co-ordinate work and comfortable timing. In these tests he worked much faster than the timing rate, which continued for three tests with the rhythm at 102 accents per min. and the work at 119 problems per min.

Observer G reported in the first of these tests that he was "adding two problems to a rhythm, but not focally aware of accents. Occasionally there is awareness of being able to go faster and of being pulled up, which is unpleasant." In the second of these tests the "chief awareness is of going on writing" and the observer is "not sure that I am keeping in rhythm". In the third test there is "awareness of doing two problems to each rhythm. I beat out the two accents with pencil in writing." But in the fourth test the observer says he becomes "aware of extra ones slipped in spaces between the rhythmic units. Then aware of going slower and feeling of ease."

So he gets the rate of the rhythm only after three tests. Evidently the double-accented rhythm was still partially extraneous to the working process during these tests, its timing only becoming definitely the working time as awareness gradually focused upon it.

Quantitative results for this observer are shown in Table II for tests from this point on, i.e. from the time Observer G 'got the rhythm'. These tests consisted of an irregular ascending series of rates administered in two days' observations. The observer reported in the first test, when the rate was 29 below his standard rate,

"great difficulty starting with rhythm. Several times I only did one to a rhythmic unit. Then for several sets I did several each. Then I took to articulating in time with the accents. I found it impossible to write in rhythm. The rhythm worries me—sometimes too slow, sometimes too fast—because I probably work in spurts. Unpleasant affect when left behind." Again this observer finds difficulty in 'getting the rhythm', and his rate is four problems less than the timing rate. This difficulty continues over into the second test of the day, with the report: "Difficult to keep in rhythm. Focal awareness of articulating figures with rhythm. Very strong tension in vocal cords as if shouting out the adding. Aware of saying them in groups of twos. No other awareness." In the next test the process is "a great deal easier, not vocalizing strongly; before articulation was with lips and now only verbalize faintly. My awareness was very focal on the rhythm with feeling that I must and it was difficult to keep with the rhythm." In the next test the work "was very smooth until nearly the end, verbalizing the sums, but less pronounced than in the last and toward the end there were extraneous verbalizations that this is easy, then made an error and spurted, putting in two extra ones". (Two additional problems were performed in this test.) In the next test, which was 25 below the observer's standard rate, the quicker pace was noticed. There was extraneous verbalization about this and the observer was "less aware of articulation and of rhythm and a feeling of following rhythm automatically". In the next test it is reported: "Beginning to feel more as it did when I was working at my own pace. Tension in hand, kinaesthesia of body, very little focal awareness of rhythm, but am aware I am writing in rhythm. Urge of rhythm is felt in tension in hand, head and shoulders. Occasionally there is a physical block and a wild feeling, then it clears." (The rate is 23 below the observer's standard.) In the next test the observer is "focally aware of the arithmetic as if it were harder to do. Vocalized adding more audibly than before and I lost rhythm." (Two less problems were performed in this test than required at a rate of 21 below the observer's standard.) The observer reports in the next test: "More effort to get the writing down, feel difficulty to keep up, once or twice a tendency to go blank, continual mental tension to keep with rhythm, aware of verbalizing sums very strongly. The awareness of being pushed was kinaesthesia in head, with unpleasant affect." With an increased rate to 17 below observer's standard there is "quite a rush to keep up".

This ended a day's observation. After practice the observer is effecting a perfect co-ordination of rate of work and rhythmic timing which has been gained with great effort and strong motivation. Vocal awareness oscillates between the rhythm and adding, and articulations increase in strength with increases in rate. There is no awareness extraneous to the working process.

With the next day's observation, at a slightly increased rate (15 below

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standard), there is greater difficulty in making the co-ordination. There is a report in this first test of

"two or three spills. To keep going I articulate figures strongly with considerable lip movement. Focal was the intent to do the arithmetic fast enough." In the next test at 11 problems below standard rate is the report, "I verbalized, I will not think of anything else, if I do I will break down. Rhythm is carried in articulation of figures and if I accent the vocalizations it helps me to concentrate on the figures. Aware of pushing myself to keep in rhythm." With a slower rate introduced at this time there is the report of "writing on the front side of the beat and verbalization, 'this is funny, this is so slow. Don't let your mind wander.'" In tests at 7 below the standard rate, there is ease in work with reports of, "irrelevant awareness such as, 'I am doing this rather well. It is easy. If you go on like this you will make an error. Attend to what you are doing.' Aware of a smoothness of writing like I got when going at a higher rate under own direction; not very aware of rhythm." In another test there is difficulty, with reports of "muscular tension in hand and arm, verbalizations to put forth as much effort as possible, extraneous verbalizations to 'get on quicker'". With increases in rates to just below this observer's standard, there are reports of "effort to get through as fast as possible, tensions, many mistakes in adding, not time to write answers, blockages, distress, worry, unpleasant affect, contraction of head and hand, breakdown". In the test in which the observer's rate is 2 above the timing there is the report of: "Rhythm went and I tried to get on as fast as I could; lost awareness of beating it out with figures." In the next test at a rate of 1 below the observer's standard rate, where there is perfect co-ordination, there are reports: "I thought once I was on top of the rhythm, then I made a mistake and I was on bottom of rhythm, then there was an awful struggle. *Even then I do not think it was as gruelling as when I worked without rhythm.* Not aware of articulating much; probably faintly done, but no lip sounds. There were one or two commands, 'You must put forth every effort', but they were not very focal."

The timing rate now is as fast as the observer's standard rate and there is perfect co-ordination in most tests and, interestingly enough, the observer reports less effort in making this record than he remembers when not co-ordinating with the rhythmic timing. Increased awareness of the working process is now reported, but none extraneous to it. This observer has effected through strong motivation and practice a perfect co-ordination of repetitive mental work and rhythmic timing at rates equal to his highest self-directed record, which was 129 problems per min.

In the next test, in which the timing is 3 above the standard rate, two less problems are performed than required by the timing and there is the report:

"Can't keep rhythm all of the time; second line, felt I must have a rest, then I missed a beat. That pause gave me a little rest. Then I had a breakdown in last 20 and lost another beat. Felt just as if I could not go on any longer. Most aware of unpleasant affect when out of rhythm." In the next test at the same rate the observer

"kept on top of rhythm, feeling it was easier. Awareness of adding and not aware of articulating sums. Faintly aware of muscular tensions all over but not focal." The observer performed perfectly and at a rate of three problems above his record. With a trial of a rate ten above this observer's standard there is the report of, "Completely broken down and only writing one figure at times to double accented rhythm. Felt I had lost rhythm and was trying to get on best I could. Towards end got hopeless." (The observer performed one problem less than his standard rate and 10 less than the timing rate.)

This observer effects a perfect co-ordination of timing rates slightly above his previous highest record of self-directed performance, which comes with focal awareness of the timing after unclear awareness of it in the early tests. There is little awareness extraneous to the working process. This observer raised his record later from 129 to 141 problems per min. at a subsequent trial in self-directed performance.

(d) *Observer H.* The highest degree of efficiency in co-ordinating the working rate and the rhythm rate is found in observer H. This observer co-ordinated his working rate with the double-accented rhythm without the loss of a beat in eighteen tests, with rates ranging in regular ascending order from a rate of 45 below the observer's standard rate (previous highest record of 153 problems per min.) to 5 above this standard. The work was performed in 2. days' observations. Six of the early tests included rates below those represented in Table II, and in these tests much awareness was reported which was extraneous to the working process. This is the only observer studied in whom extraneous conscious activity occurred in any amount during the task. Observer H reports in the first test:

"Extraneous awareness of pitch, the physical shape of the high tone, its penetrating quality, the irregularity of the rhythm. After the first line there was practically no awareness of the figures, and articulation very much in the background of awareness; awareness almost exclusively on make-up of rhythm." In the second test, the awareness of speed relations to previous test is focal almost throughout, with slight awareness of articulation of sums. In the third test the awareness is likewise largely extraneous to the working situation with the report, "Aware of ear splitting character of top note which directed my attention away and I got behind, and focal awareness of adding came in". In another test he reports, "Individual bells are not in awareness very much. Aware of grouping them and singing a tune to them. Rhythm tends to get into larger groups—into groups of 3 expressing 6 sums, which seems to lessen the intensity of the top note." There is increased "awareness of rhythm and analysis of it" in the next test and as the rate is increased to 35 below the observer's standard there is the report, "Aware of being faster. Regret that I had to wake up, made two errors. When the errors came I forgot all about the qualities of the bells, but not the timing."

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Almost all of conscious activity during these early tests is extraneous to the working process, but awareness of the working process becomes focal as the rates increase. With the lowest rate included in Table II there is the report:

"Think I was a half a beat late, but didn't miss any, a great deal more concentration, awful moments wondering if I was going to catch up, aware of kinaesthesia of body, holding of breath, and general focus of muscular attention, aware of timing when I get ahead or behind. There is a certain pleasure in concentrating at a steady level like this and not doing it in jerks. An intent was developed to concentrate more because of the faster rate." At the same rate of 31 below record there is the report of "unpleasant affect of bells, accents help to keep up speed, doesn't seem to be any motivation necessary, just happens, irrelevant awareness related to thoughts previously to reporting for observation, which were correlated with rhythm; sort of hypnotic situation" (this test was the first of a day's observation). In the next test at 29 below standard there is the report: "Discovered it makes it much easier if I look ahead instead of working on the beat. The noise quality of the high tone returned more to awareness, analysed this quality, extraneous introspection about adding process." At the same rate in another test there is a report of: "More of a set at the beginning, remembered to keep eyes ahead, no difficulty all the way through." Then there is awareness for the larger part of the test of writing movements, adding ahead, grouping of movements to get ahead of the beat for a rest pause. In the next test at increased rate there is the report of a "set to get through the whole thing without errors. Concentration on keeping eyes ahead." In the next test, 23 below standard, there is the report of "effort to catch up, no greater amount of strain to catch up. Rhythm seems to stimulate muscular co-ordination of hand. Most aware of method and speed." With the rate 17 below the standard there is the report of, "Hand didn't write fast enough. Aware in first line that this would be a greater test and that if attention lapsed, I would get caught. Distinct organic excitement. No kinaesthesia of effort. *Breath did not come in jerks as when I was pushing myself.* Attention was recalled when it started to wander." At 13 below standard there is the report, "Didn't get lost but was on verge. Difficult to keep eyes on figures ahead. No awareness of kinaesthesia but considerable organic sensation. Entirely different from unaided effort, when I am doing it myself. Occasionally an awareness of quality of bells, of pencil sliding, and of time to adjust pencil, etc. Effort to keep eye and hand together." In the next test at 9 below the record there were "a few close shaves. Very much like a very strong fear stimulus when you get behind. Certain amount of introspection, but do not recall any extraneous to the work. Awareness of method to get ahead for the change at the end of the line. Not much kinaesthesia—*certainly not to extent of work without rhythmic timing.*" In the next test, 5 below the observer's standard, there is the report, "Got mixed up, picked up again. Wrote to every accent and always got mental arithmetic, but didn't get it down legibly. At end when I was in control again wondered why I was in control, wondered why I was not as annoyed as I might have been."

These reports show Observer H developing methods in conscious activity to co-ordinate with the increased rates. Extraneous awareness—other than of methods of work—is slight with rates near the observer's

standard rate. It is interesting to have his estimate of less kinaesthesia and organic sensitivity than in working with high rates under self-direction, and reports of pleasure in co-ordinating working time and rhythmic time. Also, it is most interesting to find, particularly in this observer, that the rhythmic timing is allowed to take over the motivational function. In the next test, which is one per min. above the observer's standard, there is the report:

"Broke down at end of 4th line, kept eyes moving in rhythm but made no attempt to write the figures completely until I got control again (there are incomplete figures for 18 problems). All were performed mentally. There were questions during the first part if I could keep up. Physical tension got stronger and stronger throughout body—not in arms. When break came the movements of the hand continued a sort of dotting plan." So the working rate is definitely dominated by the rhythmic timing and there is no kinaesthesia in the arms. In the last test at a rate of 5 above standard there is the report: "Got most of them illegibly. Only missed out making some mark in two problems. Think I had correct figure in mind. Focal awareness was on mental adding except in confusions which occurred in a couple of problems and I don't know what happened. During confusion, bodily set is lost."

This observer is working at a very much higher rate than any of the other observers (158 problems per min.). He seems to perform the co-ordination of the adding function and the rhythmic timing perfectly at these high rates, but has difficulty in writing the sum. He performed 161 problems per min. at a later date under self-direction. However, it is evident that the process of co-ordination of repetitive mental work with rhythmic timing is approximately as easy for this observer at all rates possible to him as it is to pace himself, and there is evidence that it requires less effort, is more pleasant, and that he can easily take up a set in which the work is motivated by the rhythmic timing.

V. DISCUSSION OF CO-ORDINATED RHYTHMIC ACTIVITY

Throughout the reports of the four observers, whose conscious activity has been studied during the process of co-ordination of repetitive mental work and rhythmic timing, there is evidence of two separate dominating directions of awareness, that towards the objective timing and that towards the adding, and focal awareness seems to oscillate between them. There are other extraneous directions of awareness that at times become focal. This two-fold direction of awareness is guided by an intent to co-ordinate the latter with the former, and there are differences between the four observers in the development and maintenance of this intent.

Observer E planned to co-ordinate the objective timing and the adding and to catch-up, if behind, when possible. When it was not possible to catch-up, his plan was

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to lose a beat of the timing in order to perform the co-ordination. Observer F worked with a similar intent, but because of greater difficulty in holding focal awareness of the timing, his usual method was to pause the moment the rhythm was lost in awareness and wait for a beat. He seldom attempted to catch-up when behind. The intent of Observer G included much stronger motivation to keep-up, or catch-up when behind, and not to drop a beat unless it was absolutely necessary in order to get the adding written. This intent was accompanied by unusual physical effort. The intent of Observer H was never to get behind the timing and never to lose a beat of the rhythm. If anything was to be eliminated from the co-ordinated task it was to be the writing of the sums, but not the timing of mental work.

With three of the observers there is the attitude that the adding comes first, that the sums must be got down in legible form and if this cannot be done the timing must go; with Observers E, F, and G the timing is lost from awareness on occasions and the adding proceeds without it. Observer H, however, puts the timing first and abbreviates the writing process at the highest speeds. He always maintains conscious co-ordination with the timing.

The observers' reports show individual variations in the plan of work or method, in the verbalized intent, and in motor and organic motivation. It is impossible to tell how much these various factors contribute to the differences in accomplishment, but it is evident that more than initial rhythmic ability, or sense of timing, or the so-called inherited ability to follow a rhythm, is responsible for the differences recorded between these observers. Motivation in strong physical effort dominated the work of Observer G. Confidence in conscious methods was an outstanding factor in the work of Observer H. Caution and lack of confidence was expressed in the work of Observer F. These aspects of intentional activity seem to have been determiners of the co-ordination in very large degree. On the other hand, the lack of sufficient rhythmic ability may make it impossible to do what one intends to do, as is suggested in the introspections of Observer F, where awareness of the rhythm was a highly intentional awareness, which was lost when attention was focused elsewhere even for a moment. Two observers in the preliminary experiment were without sufficient ability to perform the co-ordination under the most favourable conditions.

The average results for the four observers show the fastest co-ordinated performance to be slightly slower than the fastest self-directed performance. But practice is undoubtedly very important in developing this co-ordination. The co-ordination of working rate and timing rate is a problem to be learned. Evidence of the effect of practice, defined here as repetition of an intentionally determined co-ordination, is prominent in

all reports of these observers. It may be that practice might have brought all observers up to their standard rate and above it if continued long enough.

An explanation of the slightly lower efficiency in the co-ordinated work, in comparison with the self-directed work, may be afforded by the fact that the task does not offer perfectly regular performance (totals of eight and nine are less easy than totals of two and three). A more regular task might have shown equal accomplishment under the two conditions. But it is difficult to conceive of a more regular mental task than adding problems with totals of not more than nine.

Some of the observers' reports suggest that the rhythm provided a pleasing distraction from a monotonous mental task, also that at higher rates of work there was less physical effort involved and the objective timing could be substituted for self-motivation in the performance. All observers noted that the awareness of the timing obliterated or took the place of irrelevant thinking and day-dreaming. It was suggested that self-directed speed work in a regular task such as the one used in this experiment was performed in spurts, when compared with the timed work, and that high speeds were more easily maintained in timed work when the worker was practised in the co-ordination.

VI. SUMMARY AND CONCLUSIONS

(1) While awareness of rate of objective rhythmic timing is extraneous to a repetitive mental task, as found in previous experiments (2,3), it is suggested in this experiment that many workers can, with acceptance of intent to do so, and after practice, co-ordinate a repetitive psychomotor task with rhythmic timing, such as was used in this experiment, with a high degree of efficiency and sometimes at rates equalling self-directed performance. Also, it is suggested that there will be wide individual differences in the efficiency of this co-ordinated performance, and that there may be individuals unable to perform the co-ordination even at a comfortable pace of work for them. These are suggestions from a small number of cases, which are a by-product of the study of conscious activity during the co-ordinated performance.

(2) Concerning the main purpose of the experiment, that of analysis of the conscious activity with intent to perform such a co-ordination of mental work and objective rhythmic timing, awareness is found to oscillate in focus between the rhythmic timing rate and the adding rate. This is particularly evident during the early stages of practice in the co-ordination where the timing and the performance appear to be distinct

awarenesses. There are individual differences in the degree to which the rhythmic timing assumes direction of the adding rate at any stage of practice in the co-ordination. The conscious process of co-ordination is highly individual and includes individually selected cues as guides in the performance. Also, intent is individual in the degree of importance attributed to the cues selected to perform the co-ordination. It would seem that there are degrees to which the worker can make the objective rhythmic timing a part of the conscious working situation. The conscious activity reported during difficult co-ordinated performance is entirely concerned with the process of co-ordination, while during easy performance awareness extraneous to the working process may be reported. There is a difference between individuals in the report of extraneous awareness paralleling the degree of difficulty of the co-ordinated performance. These differences seem to be related to initial rhythmic ability.

(3) The necessary psychological factors in the co-ordination of regular mental work and objective timing are indicated as follows: (a) initial rhythmic ability which is defined in performance in this experiment, (b) an intent to perform the co-ordination which is described as conscious acceptance of instructions, and (c) a conscious learning process of analysis and intentional use of cues in a plan of work, which is described in degrees of conscious focus paralleling degrees of difficulty in performance of the co-ordination. Individual differences in performance run parallel to differences in the functioning of these three factors.

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THE INFLUENCE OF THE FORM OF A PRESENTATION UPON REPRODUCTION: THE PRINCIPLE OF DETERMINATION

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- III. *Discussion and conclusions* (pp. 187-189).

I. INTRODUCTORY STATEMENT

IN the first part of this paper, it is shown that there is need for understanding the formal principles upon which clear presentation depends, and the way in which the form of a presentation affects the form of the reproduction. In the second part a research into this problem is reported—a research which suggests the importance, legitimacy and meaning of the principle of *determination*. The psychological significance of this principle is briefly discussed in the third part, together with some broad practical interpretations of it.

In so far as learning is conceived in terms of 'things or facts to be learned', there is an implied transition from 'not knowing' to 'knowing' an objectively regarded fact or number of facts. The transition itself implies a one-to-one correspondence between the facts, objectively regarded, and the 'ideas' representing the facts when learned. To bring about this transition, educational psychology has come to the aid of pedagogy by clarifying the conditions of efficient learning, and has naturally and inevitably stressed repeated presentation, effect, distribution of practices, and the like.

In such a conception of learning, there is no place for method, or form of presentation as such. The notion of discrete items of content precludes

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the legitimacy of considering formal aspects of presentation. Method, in pedagogy, therefore demands a point of view in learning other than that briefly portrayed above. Similarly, the process of remembering and applying one's knowledge demands something more than a storehouse conception of mind.

If a presentation is to be considered in terms of its form (because of the demands of 'method'), and if remembering is to be regarded in terms more dynamic and formal than mere reproduction, there is obvious need for isolating the cardinal principles of form in both cases. Since, however, presentation is of vital importance only in its dynamic relationship to remembering and applying, the type of principle needed is one that clarifies both 'presenting' and 'remembering'. Any other type of principle would, for pedagogy at least, be a mere abstraction.

II. EXPERIMENTAL STUDY OF THE PROBLEM¹

The experiments herein reported represent the beginnings of a programme of research, the object of which is to relate the formal aspects of presenting with the dynamics of remembering.

(1) *Determinate form*

Exp. 1. The purpose of the first experiment was to examine children's reproductions in order to observe and describe, if possible, that quality of form in which the reproductions might be said to conform to the presentation.

In this experiment, as in those that follow, groups of about forty-five children in the sixth year of the elementary school were asked to spend 5 min. reading a prose passage, about 300 words in length; and after an interval of 4 min., to write as much of what they had read as they could remember. Of the reproductions thus procured 816 have been used in the following reports. Different groups were used in the various experiments.

The following story illustrates the type of material used in the first experiment:

The Fishing Rod Story

Last summer Jimmy Landis was fishing from a point jutting out of the woods into a stream. He had caught half a dozen little fish and was about to cast his line for a big fellow that he thought he had seen, when accidentally the rod slipped from his hand. Rod and line were immediately caught away by the current. And the boy uttered an exclamation of chagrin at his carelessness which would cost him the loss of his best

¹ This experimental report is abstracted from a thesis (by Wees) in the University of Toronto Library. The thesis was accepted as part of the requirements for the Ph.D. degree in the University of Toronto.

fishing gear. Almost at once, however, he had a glimpse of hope, for the line caught itself on a snag in the middle of the stream about 20 ft. from where Jimmy was standing. The boy had another line with which he thought he could hook the runaway tackle and haul it in. But alas for hope! The second line proved too light to be cast against the wind. Cast it with as much force as he would, the wind caught it and threw it back. Jimmy could not swim, the water was too deep to wade, and there was no boat in which to row out to the snag. He valued his rod and was anxious not to lose it. For a few minutes he was stumped, and felt that empty vacant feeling that you have when you know you must do something quickly but you don't know what to do. Then an idea struck him. It was such a simple idea that he laughed at himself for not having thought of it immediately. To the end of the light line he tied a stone, heavy enough to give the line weight against the wind. He threw the line out across the water, hooked the floating gear, and pulled it in.

In the analysis of the reproduction of this story, four main characters of the children's work may be observed:

(a) variation and divergence in expression (omission, abridgment, elision, substitution, rearrangement);

(b) contraction of the presentation (the reproductions were not curtailed in the sense of being cut off in the middle, or even near the end);

(c) reproduction of a complete story (the boy lost his rod, and recovered it);

(d) uniformity in the selection of items that make the story complete (the children, completing the story and at the same time contracting it, selected items to carry the meaning of the whole, 56 per cent of the reproductions containing 35 common items, 35 being the mean number of items reproduced).¹

For reference purposes, the character of the reproductions which reflects the uniform selection of certain items to tell the story will be called *shape*. To indicate the individuality of expression, the variation and divergence in the use of words, we shall use the term *structure*. We may thus speak of the reproductions as characterized by individuality of structure and uniformity of shape. Since the reproductions conformed to the presentation through shape, and since conformity in reproduction is one of the common aims of teaching, the shape character was selected for further study.

Exp. 2. The purpose of the second experiment was to test the stability of shape. Evidence was secured from two sources:

(a) The Fishing Rod Story was given to a group of non-English children, for whom English was the second language. Here, increased

¹ An 'item' is any phrase that could be recognized as an attempt on the part of the child to reproduce an idea contained in the presentation.

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individuality and variation of structure was expected. Would the reproduction hold its shape under these conditions?

Evidence for greater individuality of expression than in the first experiment was observed in a 25 per cent increased contraction, five times as many errors (or changes of meaning), more various modes of expressing the same idea. In spite of this, the shapes of the two groups of reproductions (Exps. 1 and 2) were remarkably similar. For example, 89 per cent of the non-English children told a complete story, as compared with 97 per cent of the English group. A composite reproduction consisting of 23 items—the mean number for the non-English group—is almost identical with a composite constructed from the 23 items common to 72 per cent of the English-group reproductions. It may be concluded, therefore, that the shape has shown definite stability under these conditions.

(b) Further support for this point was obtained as follows: An expository paragraph on the formula for finding the circumference of a circle was presented once a day for five days, reproductions being gathered as before. Would the same shape be continued through such a series?

The data answered this question in the affirmative. The twelve ideas most frequently used in the first set of reproductions were continued as such in the succeeding sets. Other evaluations showed that the items which shaped the reproductions in the first, second, third or fourth sets were continued in succeeding sets. Thus 92 per cent of the content of the first mean composite is contained in the mean composite of the second set, 83 per cent in the third, 100 per cent in the fourth and fifth. The function of items newly included in the reproductions after representation of the material, seemed to be that of making the shape more incisive, rather than of distorting it.

(2) *Unconnected sentences*

Exp. 3. Having received assurance that shape was a character stable enough for the study of it under varying conditions, we now proposed to examine this character in order to gain insight as to the mental activity involved in reproduction. If we could procure uniformity, not of conformity, but of divergence—if we could find shapes which the children would reproduce as distortions of the presentation—we might, by studying the distortions, arrive at a principle of shape production.

It was decided, therefore, to present to the children forms which appeared to be lacking in shape, with the expectation that the children

would more or less uniformly distort the presentation away from shapelessness towards shapeliness.

The first shapeless paragraph consisted of fourteen unconnected sentences, selected arbitrarily from fourteen different and unfamiliar stories. In the reproduction of these sentences, changes among the sentence relationships were mainly of two sorts: (a) changes in the order of the sentences, (b) creation of relationships between and among sentences. With regard to the first, whatever determined the order of recall, mere sequence was obviously only one factor. The second type of change, the creation of relationships, is of greater interest to us, since it more clearly involves the active introduction of apparent shape. In the creation of these relationships, proximity appeared to be slightly active; but similarity of ideas based on familiar experience stood out much more clearly. On the whole the evidence suggested that in the reproduction of these miscellaneous sentences there was a tendency to strengthen a perceived relationship. If the relationship were one of proximity, then, in reproduction, a presented sentence that followed in time was made to follow in form as well (as by the introduction of pronominal reference, for example). If the relationship were one of form perceived, then it tended to be strengthened (by defining it, for example).

As furthering the investigation, this experiment with shapeless material indicated clearly that reproduction is a formative activity. There is an attempt to articulate the items of the presentation by relating them; and the articulation is expressed rather than implied.

(3) *Incompletely shaped form*

Exp. 4. In the previous experiment, the unconnected sentences provided reproductions of forms that were very various. A passage more intimately related within itself, though still incompletely shaped, might offer data of more uniform divergence in which the direction of the relating activity could be clearly observed.

The paragraph used for this purpose was an adaptation of "Mr Bloke's Item" from the sketch of that title in Mark Twain's *Sketches New and Old*. The presentation follows:

Mr Bloke's Item

Last evening, about six o'clock, as Mr William Schuyler, an old and respectable citizen of South Park, was leaving his residence to go down town, as has been his usual custom for many years except for only a short time in the spring of 1850, during which he had to stay in bed on account of injuries received in trying to stop a runaway horse by thoughtlessly placing himself directly behind it and throwing up his hands and shouting, which if he had done so even a single moment sooner, must certainly have

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frightened the animal still more instead of checking its speed, although unfortunate enough to himself as it was, and made sadder still and more distressing by reason of the presence of his wife's mother, who was there and saw the sad incident, notwithstanding it is at least likely, though not necessarily so, that she should be looking in another direction when incidents occur, not being lively and on the lookout, as a general thing, but even the opposite, as her own mother is said to have stated, who is no more, but died upwards of three years ago, aged eighty-six, being a Christian woman and without wickedness, as it were, or property, in consequence of the fire of 1849, which destroyed every single thing she had in the world. But such is life. Let us all take warning by this solemn occurrence, and let us endeavour so to conduct ourselves that when we come to die we can do it. Let us place our hands upon our heart, and say with earnestness and sincerity that from this day forth we will beware.

A number of points of interest in the reproductions of this passage may be reduced to three for purposes of discussion.

(1) Referring to the presentation, one observes that Mr Schuyler sets out for a walk, but, as Mark Twain says, "It don't say whatever became of Mr Schuyler." Mr Schuyler's walk is left dangling, so to speak, while the reader's attention is drawn to a distressing accident. Before events of the present are complete, events of the past intrude themselves.

The children's way of dealing with this material was to complete the events of the present out of the events of the past. For example:

Last evening about six o'clock as Mr William Schuyler was on his way down town as usual he tried to stop a runaway horse. He got behind . . . , etc.

Briefly, as Mr Schuyler was going down town the incident of the horse occurred. The first event has been completed or defined in terms of the second. Another illustration:

One day as Mr Schuyler was leaving his home to go down town he remembered something and it was he remembered in 1850 trying to stop a runaway . . . , etc.

And a third:

An old man Mr William Schuyler lived in Spring Park goes down town every night at six o'clock. He has done this every night except a few in 1850 when he was in bed trying to stop a runaway horse and got hurt. He jumped behind . . . , etc.

The proportion of children who used similar devices was 67 per cent. In these reproductions an unfinished event was brought to a termination; for this reason we may describe the process as one of *determination*.

(2) The second point toward which interest in the reproductions may be directed involves the relations included in the reproductions of the passage, "his wife's mother . . . but even the opposite". Among these items is the particular fact that the wife's mother saw the sad incident, and the general statement that "she should be looking in another direction when incidents occur". It is sufficiently interesting that the children

should have been able to extricate the latter from the surrounding tangle of words; but more important for our discussion is the use they made of it. Each of the children who used it particularized it to refer to the woman's presence on the scene, and elaborated it as if to justify and strengthen the interpretation: She failed to see the accident because she could not bear to look at it; She was brave so she did not turn away; She should have looked away because it was bad for her to see it. Sixty-two per cent of the children made such changes as these. The other 38 per cent omitted the items.

(3) Since in both the foregoing cases the direction of change is toward definition of the relationships, the term *determination*, used to denote the process in the first event, may be employed in the second as well. A third example of the delimitation of an inconsequential idea occurs in the reproduction of the end. The end may be understood to begin with "But such is life". From the group of items which include and follow this sentence the children seem to select an idea which they can interpret in terms of one or other of the two main themes of the previous passage, either the incident with the horse or the death of the grandmother. Children who may have been influenced by the former theme reconstruct the end in terms of caution:

We should clasp our hands about our hearts and promise to be careful not to be getting hurt.

Let us place our hands across our hearts and wish for no accidents.

And you should follow this one slogan Aim to be careful.

Those who take the topic of death as a suitable consequence of the preceding passage write in the following vein:

Let us try to have a Christian spirit so when death comes we will be ready for it.

We should all be thankful and be able to put our hands on our hearts and die with a smile on our face.

And I hope when we die we will die with our hands on our hearts and we will be able to say that we lived a good life such as the story talks of.

So let us remember this incident and with hand on heart look unto death. And not be afraid.

A few of the children reproduce a phrase from the end without interpretation, and a third of the whole group stopped before they reached the end.

In the present instance it appears that the determination of an idea as if to define it, to make it follow, to render it consequential, may proceed not only from the immediately preceding idea but from more or less remotely previous ideas.

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It is clear that a redirection of the relations between the rather important elements of the paragraph that have been selected for illustration would result in a general redirection of the whole reproduction. If our interpretation of the changes is accurate, the result of the determining activity should be a clearer, more direct statement of the events retailed in the presentation. Such an effect is the general impression which the reader receives from a reading of the reproductions. The more precisely directed statements of the group may be illustrated by the three following reproductions:

(1) Mr William Sc... went down town as usual. He never missed. Only last spring when he had a bad accident. He was trying to stop a runaway horse. He through himself in the back of it and yelled and through up his hands. If he had been a moment sooner he would have frightened and stopped the horse instead of making it run harder. His wifes mother was outside and saw the incident. She couldn't have been looking the other way. Her mother died three years before at the age of 86. She was a Christian and had no money nor property. It was burnt in 1849. We would take good notice of this and do as he done in trying to stop the horse.

(2) Yesterday evening at six o'clock an old man whose name was Mr William Schyler a respectable citizen of South Grove was setting in his home. it come to him that in the spring of the year 1850 he had tried to stop a runaway horse. He got behind it and tried to stop it. His wife's mother witnessed this sad occurrence. Her mother had died three years back at the age of eighty-six. She was a Christian. We should remember this occurrence because we will have to die sometime or other.

(3) One afternoon Mr Wywastle went down town which he had some business to attend to. When he was going down he was in an accident of a runaway horse. He thought the best thing to do was to try to stop it. He was behind the horse and it kicked him and he hurt his leg. His wife saw this and she looked at the opposite direction. In 1859 there was a great fire that took place. This woman had everything burned. She died upwards at the age of eighty-six. So when we are to die we are to be ready.

An examination of the concept of determination yielded by the reproductions of the Mark Twain paragraph offers the following considerations:

(1) In a presentation, a given item appears to lay down a direction which succeeding items must pursue. If the succeeding items change direction they may be altered in reproduction as if to correct the divergence.

(2) But with regard to direction, except as abstract, two points are required to fix it; one is the point of departure; the other is the end. In attempting, therefore, to redirect an ill-directed relationship, the children may be said to point it toward an end, to define or to determine it.

(3) The possibility of such determination must be a character of the first item, which therefore becomes the determinant. But since 'first' in

this case is that point at which direction is secured, the process of determination may be conceived as proceeding either from the beginning of the passage toward the end, or from the end of the presentation toward the beginning.

In these inferences one may find a reason, tentatively acceptable, for the phenomenon of divergence in reproduction: If the beginning is not a determinant of the end, or if the end is not a determination of the beginning, then either may be changed to produce a determinate relationship acceptable to the child. In this statement 'beginning' and 'end' refer not only to the first and last of the presentation as the ultimates of the determining process, but to any two or more items which stand in a determinable relationship to each other.

To test these inferences it would be necessary to vary a determinate form in such a way that recognizable divergences might be secured. The Fishing Rod story (Exp. 1) had already shown itself to be a determinate form, producing the highest degree of conformity among ten or more presentations. This presentation was therefore used for the purpose now required.

(4) *End-first form*

Exp. 5. If the inference drawn from the previous studies were correct, the simplest way to make the Fishing Rod Story indeterminate would be to place the end at the beginning, and present the form end first.¹ The story then would read:

Fishing Rod Story, End First

Then an idea struck Jimmy. It was such a simple idea that he laughed at himself for not having thought of it immediately. To the end of the light line he tied a stone, heavy enough to give the line weight against the wind. He threw the line out across the water, hooked the floating gear and pulled it in. And this is what had happened. Last summer Jimmy Landis was fishing . . . , etc.

In this form the last sentence was,

For a few minutes he was stumped, and felt that empty vacant feeling that you have when you know you must do something quickly but you don't know what to do.

The form is not entirely an artificial construct. It is analogous to a common experience in which, at a continuous cinema, a patron enters near the end of the story and sees the end before the beginning. It is

¹ Two other variations of the Fishing Rod Story were used for the same purpose, one in which the end of the story was generalized and placed at the beginning, and another in which the whole story was written as one sentence; but the results of all three devices were so similar that the report may be limited to one set of reproductions. One point of interest, however, occurred in the reproductions of the generalized beginning: most of the children particularized it and placed it at the end where it had belonged in the original.

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similar also to a 'flashback', a technique employed by some authors, in which an exciting event from the end of the story is placed first, and is followed by a chronological regression to pick up the threads leading to the event already set out. In pedagogy the form has its analogies in the frequently observed procedure by which a child is taught and is expected to learn an item independently of the form in which it is presented.

The reproductions of the end-first form show a greatly increased divergence and variation: increased contraction, a variety of beginnings and ends, major distortions of the events, reorganization of the relationships.

(1) Increased contraction: Reproduction of items was reduced by 30 per cent.

(2) Variation in the reproduced end: The original form provided reproductions of which 35 of the 36 concluded with the last sentence given in the presentation; conformity at this point, except for divergence in expression, was almost exact. But in the end-first form the children stopped at 15 different points along the passage. The form was evidently indeterminate in the sense that no uniformly acceptable termination of the story was provided.

(3) Variation and divergence in the reproduced beginning: Two main divergences occurred. Among 30 per cent of the children the beginning (originally the end) became a determinant of what followed, and the children tried to make the presented beginning the real beginning of the series of events. For example:

Then an idea struck Jimmy. And he laughed at himself for not thinking of it before. *Jimmy Landis remembered last year.* He had caught about half a dozen small fish and was casting his line for a large one he thought he saw. *He had a large stone on his line so it would go farther.* By accident the rod slipped from his hand . . . , etc. The devices by which this child tries to connect the presented beginning with the rest of the story are italicized. Her trick of tying the stone to the line was a common means used by the children to make the beginning a determinant of what followed.

The second main divergence, occurring among 22.5 per cent of the children, was to return the presented beginning to the end, where it belonged. In such case we see an instance of the backward direction of the determining process, the experience of incompleteness at the end effecting a relocation of a dislodged group of items.

In several instances (12.5 per cent) the presented beginning was omitted altogether. Apparently the children who left it out experienced it neither as a determinant nor a determination, and so could not re-

member it. A number of children were able to reproduce the beginning, but in such a way that it seemed to bear no relationship to the rest of the story. Only 20 per cent of the children wrote the beginning in a manner that could be called a reproduction of the presentation.

(4) Major distortions: In the reproduction of the original presentation only 8 per cent of the children committed error, error being defined as reorganization of items in such a way as definitely to change the meaning of one or more items. In the end-first presentation, errors occurred in 55 per cent of the reproductions. The error of tying the stone to the first rod has already been noticed. In other reproductions, as gross errors: the boy fell in, the large fish pulled him in, the boy threw the stone out to the rod, and so on. Only such errors as these were counted as errors. One interesting reproduction occurred in the case of a child who wrote, "Jimmy tied the stone on one end of the line to make the other end lighter."

The extent and kind of error in these reproductions suggest that the indeterminate form, in which items cannot be perceived readily to follow each other, had led the child in reproduction to try to make the items consequential. A merely passive retention and recall could hardly explain the 'effort after meaning' displayed in these serious attempts to tie the paragraph together.

(5) *Discussion of the foregoing experiments*

It has appeared that in reproduction children conform to some forms of presentation more readily than to others. To permit a study of the reason for divergence when divergence occurs, forms were prepared which might lead to a fairly uniform divergence from the presentation. Uniformity in divergence was observed in the reproductions of the unconnected sentences, in which relationships were created between sentences as they were reproduced (Exp. 3). In this case the direction, or function, of the relationships was difficult to identify. It seemed likely, however, that when children perceived a relationship they tried to strengthen it by introducing explicit articulation.

The direction of the relating activity was more clearly observed in the reproductions of a half-shaped form (Exp. 4). In these data the changes were more uniform, and were observed to proceed as if to render succeeding items consequential to the antecedent passage; or, to reverse the order, an item or series of items appeared to set a direction which succeeding items should pursue.

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Consolidation of these observations suggests that the child, having once obtained direction, makes an effort to maintain that direction throughout the course of the relationships. And since direction cannot be fixed, abstracted from its termini, the concept of the relating principle may be called *determination*.

The principle may be used to clarify the problem of conformity. If the perceived relationships within a form are indeterminate they may be modified in such a way as to render them determinate. These modifications, dependent on individual effort, result in a variety of reductions, rearrangements, elaborations, to which, in the first experiment, the term *structure* was applied. But in so far as the perceived relationships are determinate, modifications do not occur, and the reproduction conforms to the presentation. Such conformity, to the extent that it was uniform within a group, was called *shape*; and it may be secured in reproduction by the presentation of a form in which a direction, acceptable to the child at the beginning, is maintained to the end. This statement means that each item must be perceptible as consequent upon its antecedents and as determinant of its successors.

To test this conclusion a highly determinate form was varied in ways selected to reduce its determinate character (Exp. 5). In the reproductions a noticeable tendency to return a presentation to its original determinate form was observed. In all cases the indeterminate forms produced excessive divergence that may be interpreted as error. In most cases the divergence was obviously of the character of a determination, and in others a determination could be inferred.

(6) *Connected and disconnected facts*

Exp. 6. In order that the inferences drawn from the foregoing studies might be tested in direct relation to school work, popular classroom procedures were used in the final series of experiments.

(a) Quite a common form of presentation in schools is that of an outline statement of a series of facts. The facts, as a rule, may be perceived as logically related in varying degrees, and are to be learned by the child for test or examination purposes. Such a list, according to the data we have discussed, is a relatively indeterminate form, no matter how determinable (or logically related) are the facts set down. But into the outline list of facts, a determinate quality may be introduced by interpolating items to make the relationships explicit, to give the facts 'story

value'.¹ In the latter form, variation and divergence should be reduced, and conformity improved.

Accordingly (1) a classified list of facts about Tibet was compiled, and (2) the same facts were included in a paragraph, qualified, at least intentionally, by story value. The classified list contained 205 words, the paragraph 384. The time given for reading was the same in both cases, and the two groups of children who respectively reproduced the presentations were equivalent.

The character of the outline may be illustrated by the first few items:

Tibet—Outline

- (1) Name of country: Tibet.
 - (2) Name of people: Tibetans.
 - (3) Position of country: (a) North of India.
(b) West of China.
 - (4) Altitude: Two miles above sea-level.
 - (5) General nature of country: A plateau.
- Etc.

The presentation continued in similar form through the headings: Surface, Climate, Plant life, Industries, Capital, People, etc.

A few sentences of the paragraph will illustrate the way in which the facts of the outline were used in the second form:

Tibet—Paragraph

North of India and west of China lies a great plateau two miles above the sea. Here live the people of Tibet. Their land is a plain cut by deep rocky gorges and broken in places by immense mountain ranges choked with glaciers. In such high altitude the snows begin in September, and throughout the long cold winter the inhabitants, having neither wood nor coal, shiver over their straw fires in the filth and discomfort of their mud huts. . . . The ignorance of the Tibetans is really due to the lamas, for the priests have mercilessly murdered any traveller or missionary who might enter the country to bring news of the world outside. It is only recently that they have allowed white men to struggle up from India through the high passes of the Himalayas to catch a glimpse of this curious nation that lives among the clouds . . . , etc.

The reproductions of these two forms showed that the paragraph had a 40 per cent advantage over the outline in facts reproduced. There were four times as many errors in the outline as in the paragraph. The percentages of children reproducing the respective mean numbers of items

¹ The quality of determination might well be defined as 'story value', using the word 'story' with its original meaning of 'inquiry'. A presentation likely to be reproduced with facility would then be described as one arousing inquiry at the beginning, maintaining the inquiry through the body of the presentation, and providing an acceptable answer at the end.

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rose from 44 per cent for the outline to 53 per cent for the paragraph. In all three aspects therefore—contraction, error, variation—the paragraph showed considerably more conformity in reproduction than the outline. The most significant difference was in the number of errors, since over a quarter of the facts reproduced from the outline were incorrect, while only 7 per cent were distorted in the paragraph group.

In passing we may suggest that an important factor in decreasing error lies probably in the explicit connexion between ideas—one aspect of determination. The use of pronouns, repetitions of phrases, and so on, are definite aids in securing relationships; and these, of course, together with interpolated meanings, are omitted from the outline.¹ The fact of a common subject, given at the beginning of the outline, is probably the major factor of determination underlying the measure of conformity secured.

(b) Next, a paragraph of historical material was taken directly from an authorized history textbook. The twenty-one prominent ideas were taken from this passage, and rearranged so as to make a statement showing greater direction towards an end. Again the determinate form resulted in a greater degree of conformity in reproduction. It secured the use of more than twice the number of common items produced by the indeterminate form; it stimulated reproductions showing only one-fifth as many errors. The results were sufficiently clear-cut to warrant our inclusion here of the passages in full, and of a few examples of the weaker reproductions from both groups:

Catholic Emancipation Act: Indeterminate

King George IV of England was called the "first gentleman in Europe", and he had, no doubt, a certain external polish; but there was nothing in his character to call for our respect or admiration. He visited Ireland in 1821, and spent thirteen days in Scotland during the following year, these visits giving great satisfaction to the people of those two countries. The Test and Corporation Acts, which required that Englishmen holding public offices or a seat in parliament should be members of the Church of England, were done away with in 1828. In this way justice was done to the Roman Catholics. During this reign there was great discontent in Ireland. The Catholics

¹ In some experiments designed to discover specific aspects of *determination*, an artificial articulation was created between the fourteen unconnected sentences of Exp. 3. Pronominal reference, repetition of phrases, etc. were some of the devices employed. Or again, the sentences were presented as in Exp. 3, without articulation; but the children were told that they were all about a boy in trouble. In both these studies, the creation of relationships was decreased. It seemed that the artificial articulation had aided the children to perceive items in consequential relation to each other, and as if the common subject (boy in trouble) had provided a determinant for the sentences individually.

complained with truth that they did not receive the same just treatment as their Protestant neighbours. They were not allowed to send members of their own faith to the British parliament; and so long as this was the case, they regarded the Act of Union as a mockery. The man who now took the lead in Ireland was the celebrated Daniel O'Connell, who from his eloquence, was a favourite with his fellow-countrymen. The leading statesmen of the day could not but admit that the Catholics had a real grievance; and, in 1829, an act called the Catholic Emancipation Act was at length passed. This measure, which was carried through parliament by the Duke of Wellington and Sir Robert Peel, gave Catholics the right to sit in Parliament, and in most respects placed them on an equal footing with the Protestants.

Catholic Emancipation Act: Determinate

George IV, king of England, Ireland and Scotland a little over a hundred years ago, was a "dandy" who dressed in fine clothes and used charming manners, but who did not know how to rule his kingdom justly. At that time no Catholic could be an officer in the army or navy, a Catholic could not be mayor or alderman of a town, nor could he be elected to parliament. And when many Englishmen, including the Duke of Wellington and Sir Robert Peel, saw how unfair it was that a Catholic should not be allowed these privileges, they tried to persuade the king to sign an act granting religious freedom throughout Great Britain. But the king refused. "My father", he said, "would have laid his head on the block rather than yield, and I am equally ready to lay my head there for the same cause." George, however, was boasting. He had some care for his neck, and at the first sign of trouble he took back his braggart boast. The incident that made him change his mind occurred in Ireland. Though the Irish were mostly Roman Catholics, they could not have a Catholic to represent them in the London Parliament. This unfairness so angered one Irish Catholic, Daniel O'Connell by name, that in 1828 he got himself elected in spite of the law. Of course he was not allowed to take his seat. Then the Irish were furious. They immediately forgot that seven years previously they had cheered the king when, after a visit to Scotland, he had come to Ireland, and worn a bunch of shamrocks in his cap, and had told them with tears in his eyes that he loved them dearly. Now they were on the point of rebelling against him. If they could not have Catholic members in parliament, the Irish said, they would break away from England. George became frightened and began to listen to Wellington's advice. First he signed a bill in 1828 cancelling the laws which said that Catholics could not be army or navy officers nor officials of a town or city. Then in 1829 he signed the final act of freedom by which Catholics throughout Great Britain were granted the right to sit in parliament. A word that is sometimes used for freedom is 'emancipation', so this act that brought freedom to the Catholics was called the Catholic Emancipation Act.

The reconstruction, as observed in the foregoing presentation, was intended to set the direction of each item toward Catholic freedom. To this end considerable material was imported; without these importations, such ideas as those referring to George's character and the visits to Ireland and Scotland could with difficulty have been made determinant items in the new presentation.

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Weak examples of the reproductions of the indeterminate form

(a) King George IV visited Ireland in the year 1826. The people of Ireland like him with much satisfaction.

(b) King George IV was called the gentleman of Europe. In 1823 the church of England was done away. Ireland protested against this. The Duke of Wellington and Peel let the puritans in parliament.

(c) King George V was called English first gentleman and he was a good man and no trouble for his reign.

(d) King George the IV of England was called the greatest "gentleman of Europe" because he was all polished up. He wasn't so smart but he was a kind hearted sort of man. In his time there was the act of supremacy.

(e) King George was the man who called a meeting in about eighteenth century. It ended about in the nineteen hundred.

These examples have been described as the weakest of the group. Two or three of them probably are; but since nearly half of the reproductions were as incomplete as those used for illustration, it is difficult definitely to say that these five are the weakest of so many weak ones.

The same difficulty does not occur in the case of the determinate group. The three following examples stand out definitely as the weakest reproductions of the reconstructed form. All of the others show a more detailed understanding of the theme of the passage.

Weakest reproductions of the determinate form

(a) George IV the king of England was a 'dandy' who was well dressed and spoke well. He did not let the Catholics have their own language. He always boasted about himself and one Catholic man who could not bear it any longer gave the king some advise and he took his advise. One day he said to the people that he loved them but in his heart he did not mean this before this happened he would not let the Catholics do as they liked he said "My father knows what he does and I'm doing as my father did". Some one wanted him to sign a paper that would let the Catholics do as they pleased and he wouldn't they said to him that he would have his head cut off and he said "My father would have his head cut off but would not do as you wish and I am willing to have my neck cut off". But he was really afraid of his neck.

(b) George IV was the king of Ireland, England and Scotland. He had very good manners and like very much to boast about himself but when anything came before he was very cowardly and 1867 the people were talking of putting him off the throne because he couldn't rule and because he wouldn't sign the bill and he heard them say this and he thought he had better sign it if he wanted to stay on the throne. George IV he dressed very dainty all the time.

(c) George IV ruled hundred years ago he was very dandy and wore fine clothes. At that time no Catholic could be head of anything not even a mayor of a town. The Catholics didn't like this very much and asked George if he would let the Catholics be head of some things to. But George refused he said that his father had laid his head upon a block and he would do the same if he had to. He was only boasting when he

said this. Most of the Irish were Roman Catholic and they said they would rebel and break away from England So George gave in. And the Catholics were put at the heads of some things.

The weaker selections from the reproductions of the indeterminate form, fairly representative of a large proportion of the whole set, are evidence of the lack of comprehension produced by the first presentation. The children start out with George IV; about him they have one or two things to say; but since neither his character nor his visit to Ireland was used in the presentation to determine the rest of the passage, these abbreviated reproductions conclude with an effort at invention of something important. Even in invention the children may be seen acting on the principle of determination, each of the children constructing a consequence to his first statement: "King George V was called English first gentleman and he was a good man and no trouble for his reign."

The three selections from the reproductions of the determinate form, quite the weakest of the set, suggest a comprehension of the whole, as of a problem resolved, and only in the vagueness of their outlines show misunderstanding of the passage. This quality of wholeness, it may be remembered, was a character of shape as it was observed in the earlier studies. In these reproductions the shape is but a shadow, but shape is present, and the inclusion of only two or three accurate details would focus it to secure clarity in the outline.

It appears, therefore, in summary, that a series of chronological historical facts may be treated in such a way that they shall become more or less determinable in reproduction. By such treatment the facts become not only individually important in reproduction, but are comprehended in a conclusive whole. Under these conditions a much greater number of ideas may be reproduced with less error and with decreased variation in selection.

(c) The final experiment of the series might be said to represent a brief exploration of the possibility of further investigation by procedure similar to that used in the present study. Reproduction is a common way in which a child is expected to use an item that he has learned. But in reproduction it is intended that the child shall reproduce the item in the relations in which he perceived it. That is, the form of the presentation and the form of the reproduction are to be the same. We have seen that such a conformity is dependent on the form of the presentation.

But reproduction is only one of the ways in which items may be used. It is frequently desirable that items should be used in forms other than those in which they were first apprehended. It is indeed likely that much

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of the teacher's use of reproduction is intended not so much to fix a fact as to free it, so that it may be used in relationships other than those in which it was first observed. For such an aim the rubric, "Teach an item in the situation in which it will occur", has little to offer. The maxim stops short at conformity and offers no solution to the teacher who wishes to prepare children to manipulate relationships which only the future holds. For such a teacher the advice would perhaps be more fittingly phrased as, "Teach an item in such a way that it may be used in situations other than those in which it has occurred". The teacher's problem, then, is to find the way.

A solution of the problem, derived from the results of the present investigation, might be offered as the way of conformity. The solution would be based on the assumption that an item readily available for one form might be used with facility in another form. This assumption, however, needs validation. The problem, more directly, is this: If a presentation provides great conformity in reproduction, may an item of the form be used more freely in another form? And since conformity may be produced, according to the conclusions of our study, by presenting items consequential to each other, the question becomes one of the effect of determination on the usefulness of a given item.

A study of the problem was attempted by the presentation of two forms, one of which was the presentation (used in Exp. 2) of the circumference of a circle. This form, on the first presentation, had produced a shape more shadowy in outline than any of the score or more direct passages employed in the investigation. It was thought, therefore, that if a determinate form on the same topic could be constructed, then difference in the availability of items might be more readily observed. At the same time the topic provided a practical end, of the nature of a formula, whose use in another situation could be easily evaluated.

The determinate form was built as a narrative in which a problem occurred at the beginning of the story, in which each intermediate item was intended to lead directly from the problem to the solution, and in which the solution became the formula for finding the circumference of the circle. The presentation reads:

Circumference of a circle—narrative—A

Young Bobby was in difficulties. He had drawn a circle on a piece of cardboard to make the bottom of a jardinière in which to set his mother's flower pot; and he needed to measure the distance around the circle (the circumference) in order to find out the size of the cardboard he needed to make the sides. He had tried to take the measure-

ment with a tape measure, but he could not get the flat tape to go around in a circle. Now he was trying a string; but he would no sooner get the string adjusted in one place than it would come off in another. Just then Bobby's father raised his eyes from his newspaper. He saw the trouble young Bob was having and came over to help.

"Pop," said Bob, "how do you do this?"

"How do you do what?"

"How do you measure the distance around a circle?"

"Do you mean the circumference?" asked his father.

"Is that what you call the outside of a circle?"

"Yes."

"Well, then," said Bob, "how do you measure the circumference of a circle?"

"Well," replied his father, "first you take a ruler—"

"Ruler!" cried Bob. "How can you measure a circle with a ruler?"

"You'll see. Take your ruler and place it across the circle so that the edge runs through the centre. Got that?"

"O.K."

"How far is it from one side to the other?"

"Fourteen inches," Bob said.

"Now," directed Bob's father, "multiply the 14 inches by $22/7$. Got that?"

"Yes. $22/7 \times 14$ inches = 44 inches."

"All right. That's the circumference of the circle."

"Gee! Is that all?"

"That's all. Whenever you want to find the circumference of a circle you simply multiply the distance across the circle—that's the diameter—you multiply the diameter by $22/7$, and you've got the circumference."

The form of presentation used before, with which the above was compared, is as follows:

Circumference of a circle—expository—B

If you draw a circle on a piece of paper, the mark you make with your pencil runs around the circumference of a circle. Likewise if you encircle a jam tin or a tomato can with a piece of string, you may be said to have placed the string about the circumference of the object. If you wished, now, to measure the circumference of the circle on the paper, or to find the distance around the tomato can, you could make the measurement by running a tape measure around the circumference in the way that a tailor takes "chest measure" when a boy goes to buy a suit of clothes. In the case of the tomato can or the jam tin the measurement would be easy to make, but with the circle on the piece of paper you would probably find yourself in difficulties. It would take considerable time to place a tape measure or even a string exactly about the circumference of a circle on a paper or around any other flat circular object such as a plot of flowers in the garden. But there is a way, if you wish to use it, by which you can find the circumference of such flat objects as these very quickly. You measure the distance across the circle through the centre; then you multiply this distance by $22/7$; and the result is the measure of the circumference, the distance around the object. Thus if the distance across a round tea-tray is 14 inches, then the distance around the tray is $22/7 \times 14$ inches, or 44 inches. Since the distance across a circle through the centre

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is called the diameter, one may say, therefore, that the circumference of a circle is equal to $22/7$ times the diameter.

Using contraction and variation as indications of relative conformity, one secures the following comparative figures:

	Contraction: percentages of presentation		Variation: percent- ages of group using mean number of
	Words	Items	items
Presentation B: Indeterminate	28	20	32
Presentation A: Determinate	48	46	49

The greatly improved accuracy with the determinate form may be indicated with reference to the formula, by noting that 42 per cent of the children who reproduced the indeterminate form (Presentation B), used a formula in their reproduction, while with the determinate form (Presentation A), the formula was used in 91 per cent of the reproductions. In neither case can it be determined from the reproductions whether the children were sure of their relations in the reproduction of the formula. But from such expressions as, "You measure the circle and if it comes to 14 you multiply that by $22/7$ and you get your answer", the reader's impression from the reproductions of the indeterminate form is one of vagueness.

The children's ability to use the formula was tested by having them find the circumference of a given circle. Such a test may be considered a presentation in a new form. If in the new form the children of the determinate group should show greater facility in the use of the formula, then an indication would be secured that items reproduced with improved conformity are more readily available for use in other forms.

The test was conducted one week after the presentation. Immediately previous to the test the children were asked to reproduce again, without representation, the passage they had read. After the reproduction the procedure was as follows:

(1) A circle of radius two feet was drawn on the board, and the children were asked to write on their paper the name of the line that had been drawn.

(2) A diameter was drawn on the circle and the name of the new line was requested.

(3) Papers were distributed, on each of which had been drawn a circle, of diameter seven inches. The children were asked to find the circumference of the circle.

In the results 10 per cent of the indeterminate group replied with "circumference" to the first question, while 70 per cent of the determinate

group gave the correct reply. For "diameter" the figures were, respectively, 12 and 37 per cent.

For the third question, the use of the formula for finding the circumference, the results may be seen in the tabulation:

	Indeterminate % of children	Determinate % of children
Right procedure, right figures	14	50
Right procedure, wrong figures	17	18

The children using "right procedure, right figures", obtained the correct answer. Those who used "right procedure, wrong figures", properly measured the diameter but applied to the result the wrong fraction.

These results suggest rather clearly that a presentation which provides for conformation in reproduction provides also for the use of a determinate item in another set of analogous relationships.

Thus the reproductions of these six forms have shown that a factor of determination introduced into an outline of facts or a series of paragraphed chronological items may improve significantly the conformity of the reproductions. It has been suggested, also, that an item determined in a manner to facilitate its reproduction in one form may be transposed for use in another form with greater ease than a corresponding undetermined item.

III. DISCUSSION AND CONCLUSIONS

The psychological significance of this principle of *determination* must now be considered. This task need not involve us in lengthy discussion, since we have, in a previous publication,¹ given some treatment of it. Moreover, the essential background of our work, and the stimulation for it, comprised such outstanding and world-famous contributions as Bartlett's *Remembering* and Spearman's *The Nature of Intelligence*. We shall accordingly confine ourselves to an indication of the essential gain (to psychology and pedagogy) represented by this study.

Dynamic formalism in psychology is inevitably concerned with directional concepts. The activity represented by behaviour may be regarded as achieving, educing relationships, differentiating, energizing-after-meaning, and the like. In fact it must be so regarded. But a comprehensive and practically serviceable interpretation of activity must also include achieving-in-a-given direction. The crucial problem therefore is as to how *direction* is to be conceived.

¹ Line, W. and Wees, W. R. (1936). "Learning as doing." This *Journal*, xxvii, 162-9.

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At this point dynamics may so easily be displaced by static conceptions; formalism by notions of material or content. Thus one achieves a given, defined, rigidly interpreted goal; educes a definite relation; or differentiates particular items. After a preliminary and lengthy tirade against atomism, therefore, many writers eventually arrive at a point where the atomist often feels constrained to point out that his notions are none the less indispensable even to his most ardent critics. But even though the indisputable multiplicity of thought-objects in experience, the necessities of language, the inevitability of definiteness or concreteness in all thought and behaviour, suggest a thought manifold, an infinitely diverse array of thought content or behaviour acts, the fundamental postulate of the importance of *formal* considerations in all dynamics must never be overlooked. Direction itself must be formally conceived.

The concept 'determination' unquestionably preserves this formalism, so necessary to a dynamic outlook in psychology. While it represents a mere beginning in this most baffling field of enquiry, it none the less symbolizes the type of principle for which we must search. It makes possible prediction rather than mere description-after-the-event; for, given the determination-character of an act, the outcome can be predicted. This we have demonstrated experimentally, and the determination-character of an act applies essentially to the whole act, giving meaning to the parts, and not vice versa.

In Lewin's Topological Psychology, a similar aim is represented; but the terminology of his system has, up to now, been largely of theoretical origin, and has then been applied descriptively to observed phenomena. It has similarly been useful in setting up situations to be so observed and described. But, to our knowledge, the concepts employed have not themselves been the outcome of experimental investigations. They have not been demanded by the coming-to-light of essentially new phenomena. Furthermore, if the reasons for the topologically described aspects of a behaviour field are given, they immediately involve concepts of need, goal, and so forth—concepts which unquestionably smack somewhat of content, even though this will probably be denied by the followers of Lewin generally.

The broader significance of this principle of determination may be indicated in the field of education. While it is quite possible, even though difficult, to enrich the teacher's point of view by drawing her attention to the methodological, dynamic way of regarding presentation, the inevitable demands of examinations lend their support to the contrary—material or content—view of learning. So, too, the pupil is apt to empha-

size 'facts', 'specific goals' in his learning activities. Not only to facilitate learning, therefore, but also to preserve and continuously develop the dynamic approach in teaching, and the understanding attitude in learning, the writing of textbooks and the setting of problems must be based upon this principle of *determination*. Only thus can we hope to foster alertness and imagination in teaching, and to develop intelligent enquiry and keen enjoyment in learning.

Finally, the observation is legitimate, that while our studies have in the main confined themselves to situations involving immediate or fairly immediate reproduction, they give some indications of the importance of *determination* in educational procedures viewed from the point of view of long-term influences. Whether or not these indications will eventually lead us to the relationship between learning situations and personality development, we can at least say that to a content view of learning this relationship is entirely denied. If experimentation in the field of learning is ever to contribute to the all-important sphere of development, a dynamic formalism, particularly in its directional aspects, is essential.

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EXPERIMENTS ON ELASTICITY

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I. INTRODUCTION

WEIGHT-LIFTING, as is well known, was employed by Weber, Fechner, and G. E. Muller, and is still widely used, in experiments on the psychophysical methods. But it was for the main part merely a means to the end of these methods, and little attempt has been made to examine the psychological factors which enter into our perception of weight lifting. More attention was given to the latter by Katz⁽¹⁾ in his experiments on persons whose limbs had been amputated, and by Fischel⁽²⁾ in his paper on the phenomena of 'weight transformation'. The present authors have been led to conduct experiments in this field, at the senior author's suggestion, the essential interest being also in the psychological phenomena. The general problem is that of analysing the impressions which are brought about by pulling or pushing at elastic bodies such as springs and elastic bands. In the present paper we measure the impression of *weight* in such a pull, by matching it with the impression of normally lifted weight. The paper is concerned with objective data, and it is left for other papers to deal more thoroughly and individually with a description of the phenomena of weight lifting and elasticity perception.

II. GENERAL EXPERIMENTAL CONDITIONS

In each of the experiments to be reported, subjects either lifted a weight, or pulled an elastic band or expanding spiral spring, weights, bands and springs being attached to wood handles of uniform size and design. The handles were grasped palm downwards, one hand being used at a time, and the subject made vertical lifts or pulls. As far as possible lifts and pulls were made under comparable conditions as to direction, manner of hand grip and arm movement. All subjects made short up-and-down excursions while lifting and while pulling, as is done normally when one 'feels' the weight of an object; that is, judgements were made under conditions of movement, and each person could perceive when a weight was being lifted, and when a spring or elastic was being pulled. Otherwise the usual details of the psychophysical methods were followed.

III. EXPERIMENT I. LIFTED WEIGHT AS STANDARD

(1) *Method*

As standard we used a weight can of 2.50 kg. weight, the variable being a wire expanding spiral spring. Using the method of mean error, subjects were required to match the impression obtained by lifting the weight with a pull on the spring.

The spring was rigidly fixed at its base, and a wood handle, of the same size as that on the weight can, was looped firmly into its upper end. A pointer, fixed to the top of the spring, ran freely alongside a vertical millimetre scale, and allowed us to read the extensions of the spring. The objective weight required to give these extensions was determined by calibration with a spring balance, which in turn, of course, is calibrated in terms of weight.

Each subject (twenty in all) made thirty-two comparisons with the standard lifted weight, beginning the pull at zero in sixteen trials, and with a pull of 8 kg. (approx.) in the other sixteen trials, left and right hands each making an equal number of pulls. The subject stood with his feet near the base of the spring, one foot at each side of it, and looked straight forward, the can and the spring handle being handed to him by the experimenter. The directions were: "First perceive the weight of the can, and then match that weight impression with a pull on the spring, using the same hand. Pull the spring until you feel that the *weight* you then perceive matches the weight of the standard can."

(2) *Results*

The results for twenty persons are summarized in Table I, which shows at (i) the mean of each person's thirty-two pulls, each a match for the impression of weight given by lifting the 2.50 kg. standard can. The standard deviation of each person's pulls is given at (ii). Person B.C.F.; for example, matches the standard lift with a pull on the spring of 6.50 kg. (s.d. 0.70), an amount 2.6 times as great as the standard weight.

Table I. *Showing the mean and standard deviation of 32 pulls for each subject, matching for weight impression a 2.50 kg. weight*

	Mean kg.	S.D.		Mean kg.	S.D.
Subject	(i)	(ii)	Subject	(i)	(ii)
B.C.F.	6.50	0.70	A.M.R.	3.82	0.55
O.	5.50	0.55	P.	3.80	0.50
R.	4.50	0.36	M.B.R.	3.70	0.35
J.	4.42	0.41	E.R.	3.65	0.30
W.S.	4.32	0.46	B.	3.40	0.42
W.R.	4.25	0.40	J.L.	2.98	0.36
J.W.	4.21	0.41	H.	2.96	0.36
W.	4.10	0.39	F.	2.93	0.43
J.C.	4.00	0.44	F.C.	2.82	0.33
C.R.	4.00	0.30	B.K.	2.70	0.32

The average pull of all twenty subjects is 3.92 kg., an amount nearly 60 per cent in excess of the standard.

The standard deviation values are reported to give only an approximate idea of each person's dispersion. The distributions tend to be skewed, and only on rare occasions was a pull on the spring as low as 2.50 kg.

IV. EXPERIMENT II. ELASTIC PULLS AS STANDARDS

(1) *Method*

In this case we used an elastic pull as standard, for comparison with a lifted weight selected from a series of weights, the latter constituting the variable. The psychophysical method was that of approximate limits.

Six standard elastic pulls were employed. They were constructed of elastic bands, of the kind commonly sold by stationers, fixed over a hook at their base and to a wood handle of the usual standard kind at their upper end. The six pulls contained 1, 3, 6, 9, 12, and 15 similar elastic bands, that is, in effect, 2, 6, 12, 18, 24, and 30 elastic strands respectively. A standard pull at any one of these six consisted of grasping the handle with the preferred hand (used throughout), palm downwards, and then pulling the elastics smoothly upwards, in a vertical direction, through a height of 10 in. The correct height was registered when the back of the

subject's hand touched a string which was stretched horizontally at a height of approximately 11 in. (allowance being made for the thickness of the subject's hand) above the zero pull position of the elastics. The six pulls were measured by a spring balance, and were 0.55, 1.60, 3.30, 4.75, 6.30 and 9.0 kg. respectively. These are shown by curve I in Fig. 1.

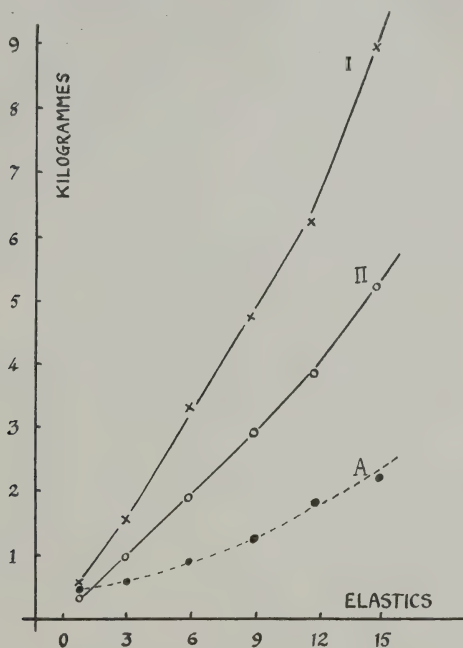


Fig. 1.

The weight series consisted of seventeen cans, of weights 0.25, 0.50, 0.75, 1.00, and then upwards by 0.50 to 6.0, followed by 7.0, 8.0 and 10.0 kg.

The subject stood, facing a low table on which were the weight cans, and along the front of which were screwed the hooks holding the elastic bands. He was handed, say, the 0.55 kg. elastic, and made the standard 10 in. pull with it. The weight impression so obtained, at the 10 in. extension, had to be matched by selecting a weight from the variable

series of cans. These were handed to the subject one by one, each judgement being prefaced by a fresh pull at the standard. The subject was allowed to make a judgement intermediate in amount between any of the given weights.

(2) *Results.*

The averaged results for twenty men, not those used in Exp. I, were as follows: The pulls of amounts 0.55, 1.60, 3.30, 4.75, 6.30, 9.00 kg. were matched by weights of amounts 0.35, 0.95, 1.90, 2.93, 3.82, 5.22 kg. respectively, that is, in each case by a weight which is approximately three-fifths of the actual pull in the standards. The results are shown diagrammatically in Fig. 1. Curve I shows the pull in kilograms plotted against the number of elastic bands in the pull; curve II shows the weight judged to be equal to that perceived in the aforementioned pulls. No person ever gave a judgement that came within range of curve I, and one person gave the result shown by curve A, in which the 9.0 kg. pull is equated by a weight impression of only 2.2 kg., and *pro rata* for the other pulls.

V. CONCLUSION

The impression of weight in a pulled elastic or spring is considerably less, according to the above experiments, than the actual weight required to give that pull. The underestimation of the pull in terms of weight perception is of the same order whether the subject begins with a zero pull in the spring, gradually increasing this until a match with the lifted weight impression is obtained, or whether he begins with a pull of the order 8.00 kg., gradually decreasing this until the required match is supplied. The result is similar, as Exp. II shows, when a pull is used as standard for comparison with lifted weights. It required a spring balance to convince most subjects that they had equated a pull with a lifted weight of the order 60 per cent of the pull.

This difference between the effects of a pull and a lift points to phenomenological differences in the impressions of weight and elasticity; these, and further experiments in this field, will be reported later.

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A FIELD EXPERIMENT IN RACIAL¹ PSYCHOLOGY²

BY S. F. NADEL

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I. THE PROBLEM

IN the present article I shall describe certain attempts at attacking, experimentally, and in the field of so-called primitive cultures, the problem of a possible correlation between diversity of culture and psychological differentiation. This investigation differs essentially from the psychological experiments on the mentality of primitive peoples which have been carried out so far. Most of these experiments were devoted to the study of lower mental processes and to the measurement of psychophysical reactions. Where higher mental functions were involved, as in the investigations of Porteus⁽¹⁾ and Garth⁽²⁾, the main aim was still a quantitative analysis of psychological responses with respect to some

¹ The term 'racial' in the title needs explanation: it has been used here simply for want of a better term. It refers here not to racial groups in the strict sense but to groups which, in loose terminology, are generally referred to as tribes, peoples, or communities, i.e. cultural groups. The term culture-psychology, however, with its philosophic connotation, does not recommend itself.

² A paper read at the Manchester meeting of the British Psychological Society, April 1937.

universal scale of mental characteristics or abilities. The present investigation is in no way concerned with measuring or rating psychological reactions and characteristics. It is devoted to a qualitative analysis, more precisely: a qualitative comparative analysis, of the higher mental processes—perception, memory and thinking processes—of two primitive peoples. Its primary aim is to discover ‘typical’ psychological traits among groups of different culture, in other words, to study by means of a psychological experiment what Professor Bartlett calls the “preferred, persistent tendencies of groups” (3, p. 255).

During two anthropological expeditions to West Africa (Northern Nigeria) I had made some effort to combine anthropological work with experimental psychological studies. I had been working for some time among two neighbouring tribes, the Yoruba and the Nupe. These, living side by side, in the same material environment, under the same general conditions and at the same stage of civilization, yet present a picture of two entirely different, almost antagonistic cultures. This seemed an ideal situation for an investigation on the lines of such ‘racial’ or culture-psychology. Thus I carried out the same series of experiments among both groups, and I shall describe in the following pages some of the results of this comparative study.

II. THE EXPERIMENTS

I propose to describe two sets of experiments which correspond very closely to the type of experiment used by Bartlett, namely experiments on *Description* and *Repeated Reproduction* of pictures and stories (3, p. 45 *et seq.*). The pictures were photographs of objects or scenes with which the experimental subjects were all familiar. Some of them were enlargements of my own photographs taken in the country, and some were taken from the *Illustrated London News* and other illustrated papers. The story which was used I composed myself. It was modelled on a native folk-tale of a type fairly common both among Nupe and Yoruba, but with a view to bringing out certain psychological test facts. The experiments were, for practical reasons, carried out in schools, namely in the Government Middle Schools in which teaching is done by native teachers and to a large extent in the vernacular. The two series which I am describing here were done with 20 students from each community, of the age of 15–18. All experiments were done in the vernacular; among the Nupe I myself carried out the experiments, as I was speaking the language fluently; among the other group, the Yoruba, I trained two teachers to carry out the experiments for me.

The present study can only claim to be a first attempt in field experiments of this kind, carried out under conditions which did not allow a more extensive investigation. Yet the question could be raised how far the evidence obtained in experiments which were carried out on so small a scale and in so special a setting can be considered significant and representative of the culture at large. In order to secure a representative group I selected for my experimental subjects students who came from a great many different places in Nupe and Yoruba respectively. The two experimental groups were matched as closely as possible with regard to age, school standard and the surroundings from which they came. In Nupe I was able to work with larger groups: with 60 boys of the age of 12-18 who were taken from every school form, i.e. who had been students of the Middle School for periods varying from 1 to 5 years. Now, as I shall show later, the experiments admit to a certain extent of statistical interpretation, i.e. the responses to certain essential features in the story or the pictures can be classified and treated quantitatively. This statistical evidence allows us to gauge the extent to which the 20 Yoruba subjects can be considered a significant and representative sample. I found that the means for the various responses calculated for the small group correspond very closely to the means arrived at for the large group; in other words, there exist no unequally distributed marked individual differences which would invalidate the results obtained among the small group. Since school standard and age do not correspond very closely in Nigerian schools, the statistical evidence can moreover be used to check independently the influence of age and educational standard. As regards the age factor, we find a slight rise in the appreciation of factors of reasoning and emotional facts corresponding to age; a fairly unchanged appreciation of situational facts; and a fall in the appreciation of moral ideas from the age of 13-15. More important for this investigation is the educational factor; here the statistical evidence proves conclusively that the difference in educational standard has no influence on the psychological responses and attitudes examined in these experiments.

III. THE CULTURE DIFFERENCES

Before going into details I must say a few words about the two cultures and the nature of their diversity. The two groups speak different languages which, however, belong to the same linguistic group (of so-called *Sudanic* languages). Their economic system and their general social organization are similar. Yet other aspects of their culture, aspects which may be held to express more specifically psychological trends or tendencies,¹ reveal a fundamental difference. The religion of the Yoruba is characterized by an elaborate and rationalized hierarchical system of deities, each of which has its specific, specialized duty and function. The Nupe have no such system; their religious beliefs centre round a concept of magic of the 'mana' type, i.e. vague, abstract, impersonal power. In the rich, highly developed realistic art of the Yoruba the main *motif* of pictures, plastics and wooden images is the human figure; religious and mythical emblems of this kind play an important part in their life. Again,

¹ See my article on "The Typological Approach to Culture"(4).

the Nupe have nothing of the sort, their art is imageless, they have but crude wall paintings, and their strength lies in ornamental, decorative art. Also the Yoruba have a strong sense for pantomime and drama, for which again there is no parallel among the Nupe.

IV. THE STORY EXPERIMENT

The story was read to the students at 8 a.m., i.e. during their first class. At noon they were asked to relate the story for the first time (each boy by himself) and after one week for the second time. Each time the leader of the experiment wrote down the story as it was told. The story (in free translation) runs as follows:

Long, long ago there were a man and his wife. They had two children, sons. When these sons had grown up, they saw a beautiful girl. They both made friends with her, and loved her very much. They loved her with one heart. But the girl did not love the elder brother, she only loved the younger. Thereupon the elder brother went to the younger and told him to give up the girl. He said: "I am the one to marry the girl for I am your elder brother." Thus he spoke. But the younger refused. They quarrelled a great deal till the elder brother became furious. When the night fell he went to the hut where the younger brother was sleeping and killed him with his sword, he killed him with one stroke. When the people heard the news they said: "He did an evil deed, God will revenge it." But the girl cried, she cried for twenty days and her heart was full of pain. When she had finished crying she left and went to another place. Nobody saw her again.

The responses which the contents and structure of the story evoke in the experimental subjects can be analysed under four or five different headings: There are points in the story which stress emotional reaction; points which bear on moral ideas; and points which appeal more specifically to reasoning, i.e. crucial points in the logical progress of the narrative. The text also contains certain set phrases and formulae; and, finally, a number of situational or circumstantial details. Obviously, these different categories in which the features of the narrative can be grouped must overlap to a certain extent. But it is just this overlapping, the application of different and even divergent aspects of interpretation to the same features, which will reveal to us most strikingly the different psychological attitudes with which the two groups approach the data presented in the narrative.

(1) *Set phrases and formulae*

The beginning and the end of the narrative, and one or two other phrases, represent more or less fixed formulae typical of all such stories. These phrases were in most cases repeated literally, both by Nupe and

Yoruba. In the reproduction of one formula-like phrase, however, we grasp a first striking difference between the two groups. The phrase in question is the sentence "God will revenge it", spoken in the story by the people when they learn of the crime. This set phrase, which is commonly used in a context of this kind, is repeated verbally by 10 Nupe (13 in the second recall); 6 (3) Nupe leave it out altogether; and 4 (4) substitute another sentence of the same meaning. Among the Yoruba 5 (5) quote literally; 3 (1) leave it out; and 12 (14) substitute another sentence. Yet the new sentence which the Yoruba substitute does not merely reproduce, with different words, the same meaning. In 5 (10) Yoruba reproductions the changed phrase introduces an altogether new element, namely a rational explanation of the expression "God will revenge it", uttered under these particular circumstances. Thus (Y 8), in the first recall, lets the people speak as follows: "The elder brother has done wrong; we cannot revenge it, God will revenge it." In the second recall this reasoning is more cogent still: "The people said: 'They should not do anything about it; God will punish him.'" Or (Y 12): "They said, 'One should leave the elder brother alone. God will judge him.'" Or (Y 15): "His sin is very grave; no one but God can judge him." Only two Nupe in the second recall (none in the first) felt it necessary to supply this additional rational link which turns a stereotyped phrase, quoted almost automatically, into a logically sound judgment.

(2) *Rational aspects*

In the stress which is laid upon such rational links and logical elements lies in fact the most fundamental difference between the Nupe and Yoruba version of the narrative. A characteristic example is the reproduction of the third sentence of the story ("when these sons had grown up"), a 'crucial' sentence in so far as it supplies an essential rational link in the narrative; obviously the brothers must have grown into men before they could have had their love affair. Now among the Nupe only 3 mentioned this fact in the first, and 4 in the second recall. The corresponding figures among the Yoruba are 17 and 18. But even the most salient and most essential points in the story, the preference of the girl for the younger brother and his refusal to renounce his claims, points which are missing in none of the Yoruba reproductions, are neglected by the Nupe: 9 Nupe in the first and 5 in the second recall leave out one or the other of these facts on which the whole development of the story depends. Similarly, the quarrel between the two brothers which supplies the motive for the murder is mentioned by 3 (3) Nupe only as against 12 (14) Yoruba.

But, as we have seen already, the Yoruba do more than merely observe the existing rational connexions in the story. They introduce new ones, or try to render existing connexions more striking. For instance, towards the end of the story there is the sentence "when the girl had finished crying, she left the town". Among the Nupe 19 (16) reproductions include a sentence the meaning of which closely corresponds to the original; among the Yoruba 2 only kept to this rather weak connecting link; 7 (8) placed the two sentences simply side by side: "she wept for twenty days, and then she ran away"; and 10 (9) turned the connexion into a sentence such as this: "when she thought of all this sadness that existed in this matter, she left the town." Now the most interesting fact is that this sentence is a typical expression of Nupe 'mentality'. When I first composed the story I had placed the two sentences side by side: "the girl cried for twenty days. Then she left the town." But my two Nupe informants, with whom I worked out the text, objected to this expression, and insisted on bringing in the sentence "when she had finished crying". This sentence was then simply taken over into the Yoruba text. But in the experiments most of the Yoruba rejected it as we saw, left it out or, more characteristically, reformulated it, thus strengthening the logical cohesion of the story.

One could try to establish statistically this difference with respect to the stress laid on elements of reasoning. Contained in the story are four sentences which represent specifically rational or logical links. Every rational link which appears in the reproductions, both an original rational link which was already contained in the story and any new, additional 'rationalization', is scored as 1. The means for the 20 experimental subjects then amount, among the Nupe, to 2.3 (2.4), and among the Yoruba to 5.5 (6.0), i.e. the Yoruba figure is more than twice the Nupe figure, and considerably above the theoretical value of 4.

(3) *Situational aspects*

The retelling of the Nupe, although neglectful of the rational connexions embodied in the text, does itself introduce certain new, additional links and connexions. These links stress situational, circumstantial facts, details of place and time, which are of little importance for the logical progress of the story. They may amount only to irrelevant remarks, filled in between sentences, such as "this went on for some time", or "thus they lived", etc. Or they may completely replace the logical connexion, as in the version of (N 7) who, leaving out the quarrel of the two brothers, jumps from the statement about the girl's preference for the

younger brother to the scene of the murder, by simply stating: "one day, at night, the elder went to the sleeping hut of the younger..."

In other cases, however, the situational and temporal details tend to build up a vivid, concrete picture of the happenings in the story. Thus, for example, 4 Nupe (6 in the second recall) mention that the brothers were 'travelling about' when they saw the girl; 3 (5) let the brothers meet the girl 'in town'; (N 25) adds the information that the brothers 'afterwards returned home'. And after describing the murder 3 (6) Nupe continue the story as follows: "On the morrow", or "when it became day, the people heard of the crime." Yet these connexions never strengthen the cohesion of the narrative as such. On the contrary, one gains the impression that the story dissolves itself into a number of different scenes and situations.

This difference between the tense, logically convincing way in which the Yoruba reproduce the story, and the loosely knit Nupe way of retelling which is concerned more with details of situation and temporal sequence, becomes most evident in connexion with the 'situational' item in the story, that the murder was done 'at night'. Among the Nupe this is in itself an important feature which is given a prominent place in the reproduction. Thus:

(N 54) The younger brother refused to leave the girl. The elder left the matter then. Till one day, at night,...

(N 60) The younger brother said: "No." Then it became night. The younger brother slept in his hut. The elder took his sword, and killed him on that night.

But among the Yoruba the reference to the night is clearly a minor detail which is pushed into a relative sentence and is quite overshadowed by the really relevant fact, which is the connexion between the anger of the elder brother and the murder.

(Y 6) The two brothers quarrelled terribly. The elder pulled out his sword and went to kill the younger brother where he slept.

(Y 9) The brothers quarrelled, and when night came the elder took his sword. He went and cut off the head of the younger brother because he had spoiled his heart.

(Y 15) They quarrelled about the girl. So much that at night the elder took his sword, went to the place where the younger slept and attacked him with his sword till he killed him.

(4) *Emotional and moral aspects*

In this experiment, rather surprisingly, the emotional factor seems to go hand in hand with the reasoning factor. Emotional and rational aspects do not seem to represent divergent or even independent principles; the more 'rational' Yoruba stress at the same time emotional reaction; and

the Nupe, neglecting rational connexions, seem to neglect the emotional aspect as well. The explanation is that the facts and events which bear on emotional reactions may be interpreted as rational motivations of the acts related in the story. This is in fact how the Yoruba approach, and utilize, the emotional aspects of the narrative.

Yet the comparative weakness of the emotional factor in the Nupe reproduction is not due to the Nupe lack of appreciation for rational connexions. The emphasis on emotional factors as far as it is evident in the Nupe version is not correlated with factors of reasoning; the appreciation of such elements as the beauty of the girl, her sadness after the murder, or the strength of the brothers' love, is not turned at once (as is the tendency among the Yoruba) into an interpretation of motives and reasons, but seems to represent a specific, self-sufficient trait. To a certain extent the emotional response among the Nupe appears linked with the tendency to dissolve the narrative into a series of concrete situations. (N 25) is a good example: he mentions that the brothers 'returned home' after they met the girl; he reproduces the girl's avowal of her love for the younger brother in the wrong place, but again in a typical 'situational' context: "when the girl learned of the murder, she said that she had only loved the younger brother"; he lets the people speak as follows: "This is what he did, God will revenge it." But he repeats twice how strongly the brothers loved the girl, and although he does not connect the girl's sadness with her disappearance, he does not omit, as many other Nupe did, the girl's 'aching heart'. But the cases in which I could find a similar correlation are few, and it seems possible that the emotional bias among the Nupe constitutes an independent, secondary psychological characteristic which may only run parallel to what we discovered to be the dominant trait in Nupe reproduction: stress on situational and temporal connexions.

The response to the moral ideas which are referred to in the narrative is again bound up with rational factors. The reinterpretation and rationalization of the statement "God will revenge it" which we discussed above is a point in question. But the attitude to the moral statements adds a new characteristic to the psychological differentiation of the two groups. All the Nupe reproductions keep closely to the meaning of the sentence "he has done an evil deed". The Yoruba reproductions on the other hand are much more varied and depart much more freely from the original. Here are a few Yoruba versions:

First recall:

(Y 6) The people said: "he abused his power over the younger brother."

(Y 12) It surprised the people (to hear) what the elder brother had done. They said, "one should leave him alone, God will punish him."

(Y 18) When the people learned of the deed, it pained them very much. They said: "God will revenge it."

Second recall:

(Y 1) The people judged the elder guilty and evil; but God would punish him.

(Y 2) and (Y 5) The people were very sorry for the boy, but (they said): "Our Lord God will punish him."

(Y 12) The people said: "The elder should have left the younger alone. God will punish him."

To the Nupe then, the unspecific and abstract reference to the wickedness of the deed seems natural and satisfactory. The vague, schematic connexion which the story establishes between the wrong deed and the supernatural punishment, seems to satisfy their attitude towards moral value. The Yoruba, on the other hand, feel the urge to define the nature of the evil deed more closely and more explicitly. They weigh specific deed against specific punishment. Their attitude to the moral judgment is the same as to all the other features of the story: rational, and intent on clear logical connexions.

(5) *Arrangement and order*

It is interesting to study the question of the order in which the events of the story are related and remembered. In the first recall the Nupe frequently (in 11 cases) rearrange this order, but only 2 Yoruba do the same. The explanation, however, cannot be that the precise, logically coherent narrative keeps more closely to the original story, for in the second recall the figures for the rearrangement of order are equal in both groups: i.e. the Nupe figures rise by 5, to 16, and the Yoruba figures by 14, from 2 to 16. Evidently, Yoruba recall, relying more on logical connexion, is apt to abandon the external form, whereas the loose, devious way of Nupe retelling which in the second recall is not much more free than in the first, may be more tied to the external order of events as to the only framework establishing coherence.

Before I pass to the picture experiments I will quote two full stories, one from the Nupe and one from the Yoruba, to illustrate the fundamentally different attitudes with which the two groups approach their task.¹

(N 11) Long ago, there lived a young man. He had a brother. They were elder and younger brother. They lived, they lived, till they saw a young girl. The girl was very beautiful, they loved her with one heart, both the elder and younger brother loved her.

¹ Other full quotations of the story will be found in *Africa* (5).

One day the elder said to the younger he should give the girl up, "because I am the elder". The younger brother said he would not give her up. Although the other was the elder he would not give her up. But the elder brother left the matter then, till night fell, then he went to the hut where the younger brother slept and killed him with a sword. When the girl saw this she cried for twenty days and her heart ached. The people who heard this said, what he had done was bad, God will revenge it. When the girl had finished crying she went to another place, nobody saw her again.

(Y 13) Long ago, there lived a man who had a wife. They had two children, sons. When they grew up they saw a girl who was very beautiful. They loved her, but the girl loved the younger more than the elder. When the elder brother realized this he said to the younger: "You must give up the girl, for I am your elder brother and I ought to marry her." When he said this the younger refused and they quarrelled so much that the elder brother took his sword, went to the younger, attacked him with his sword and killed him. When all the people heard this they said: "An evil deed has been done by the elder brother." They said: "Nobody should touch him, for God will judge." The girl cried for twenty days. Her sadness was so great that she left the town, and nobody saw her again.

V. THE PICTURE EXPERIMENTS

I showed six pictures of different topics which varied in complication and distinctness. Each picture was exposed for about 10-15 sec. The experimental subjects were asked first to write down (in vernacular) what they had seen, after every picture. After a week's time they were asked to relate what they remembered, and this was written down by the leader of the experiment. The photographs represented:

- (1) Two leopards in the bush.
- (2) A black horse with a native groom in European garb, in an open field.
- (3) A native in Mohammedan dress (which is common everywhere in Northern Nigeria), sitting on a table on which stands a native clay lamp.
- (4) An oasis in the desert, showing a camel in front of a date-palm and two Arabs. (This was a rather dark and indistinct picture.)
- (5) A mosque with a huge crowd of people in front. (A clear but somewhat complicated picture.)
- (6) A bush fire (dark, very indistinct, in some respects corresponding to the familiar 'ink-blot' picture).

(1) *The background of experience*

A first difference in the reaction of the two groups is due to the fact that in the description or interpretation of certain features in the pictures the different background of knowledge and practical experience characteristic of each group comes into play. Although the general cultural background of the two groups is the same, there exist naturally certain specific details of culture which are different in the two groups, and which to a certain extent enter into the description of the pictures. Thus quite a number of Yoruba called the clay lamp in the third picture a 'pump'. This

rather surprising interpretation became clear in the explanation which one Yoruba gave; he described the object as a "clay lamp with a handle like the pump at the railway station". Ilorin, where the Yoruba experiments were made, is on the railway; the place where the Nupe experiments were made is not, and this explains why this particular interpretation occurred only among one group. Among the Nupe we have, instead of the 'pump', a 'bicycle wheel', or the 'handle of a motor-car'. Both Nupe and Yoruba seem to derive satisfaction from bringing in these very special, and up-to-date *motifs* of interpretation. The *motifs* contributed by this specialized knowledge tend, however, to disappear in the second recall and to be replaced by an interpretation of a more general character. For instance, there were 6 'pumps' in the first description but only 3 in the recall: one 'pump' was replaced by the ordinary 'clay lamp', one was left out altogether, and one reappeared in the following disguise: "a man kneels down (obviously for prayer) under a tree which bends down like the arm of a water-pump."

But the real, psychologically relevant, difference between the two groups does not lie in the different material or content of experience which is utilized in the description, but in the different psychological organization of this material. Among the Yoruba, to start with, the details contributed by special knowledge serve as centres for an interpretation which endeavours to lend a unitary and consistent *meaning* to the whole picture. A few examples will explain what I mean.

(Y 3) An old man wearing turban and white gown, kneeling near something that looks like a pump which has a hole and is bent. The man is repairing the bottom of the pump.

(Y 6) A blind man, wearing a turban and big white gown, pumps water from a fountain in order to drink it.

In the recall we may find a more or less complete reinterpretation: the first example brings the tree which looks like a pump, and the second gains completely new shape:

(Y 6) A Mohammedan teacher, wearing white turban, sits and lights a lamp in front of him, he places it on his right-hand side (obviously in order to read).

We see that, although there are changes from description to recall, the factor of consistent meaning still determines the interpretation. Now among the Nupe there is surprisingly little of such attempts to read a meaning into the picture.

(N 2) A man is kneeling, he wears a beautiful white turban, his eyes are closed. There is something in front of him which looks like the handle of a motor-car.

(N 12) There is an old man who has only one eye. He is sitting and doing something with a bicycle wheel.

Certain attempts at a consistent, unitary meaning, do exist also among the Nupe. For instance:

(N 3) A man with a waterjug, he is holding it and making his ablutions in a hut.

(N 10) A man with a white turban is sitting, his forehead bent, worshipping an idol.

But these meaning-oriented interpretations are (1) in the minority; (2) the consistency is not established really fully and satisfactorily: e.g. the 'man in turban' would be a Mohammedan; still he is said to worship an idol; (3) the meaning evaporates in the recall. Here are the reproductions of the two meaning-oriented Nupe descriptions which I have just quoted:

(N 3) A man sitting in a beautiful room, with a big water-jug, he is crouching.

(N 10) A man with white turban is sitting, he is crouching, he is watching something in front of him which looks like a motor-part.

(2) *Imagination*

From the examples quoted above it will be seen that the positive or negative part which the meaning factor plays is not correlated with predominance or lack of imaginary addition. Quantitatively, the contribution of imagination is the same in either group. The difference lies again in the part which imaginary details or additions play in the organization of the material. Among the Nupe they just constitute additional items and there is no tendency to weld all the details into a unitary interpretation. Thus, in the picture of the horse and the groom:

(N 13) There is a European horse standing together with a European near a small hill (!)¹ A few men stand behind the European (!), they look like soldiers and are coming towards him.

Among the Yoruba such mere addition is the exception. The rule is represented in the following example:

(Y 13) A horse near a river (!). It looks as if it is going to be washed. It has a bridle in its mouth, and the man who brings it to the river stands with the horse, he does not hold it but only looks at the horse.

In the recall the orientation towards meaning is intensified by introducing still another imaginary detail:

(Y 13) A man taking a horse to the stream. He stands there trying to pull the horse by the rope (!), but the horse does not move away from the stream. The man is watching the horse.

(3) *Meaning-oriented response*

The essential difference in the psychological responses of the two

¹ Exclamation marks indicate imaginary additions.

groups lies then in this: the Yoruba are meaning-oriented; they weave into most of their descriptions and recalls a unitary meaning; the Nupe tend to present the facts which they notice or remember in a purely enumerative manner. Now, *meaning* is, generally speaking, a recognition of relation, more particularly of a relation bearing on purpose and causality. In the picture of the horse, for example, only 6 Nupe descriptions and 3 recalls (against 15 Yoruba) attempt to establish the simplest relation on which a unitary meaning could be based, namely a relation which accounts for the presence of the two figures in the picture, the horse and the man; i.e. describes them as a horse and its master, or a horse and its groom, etc. Or, in the picture of the two leopards: 10 Yoruba read a meaning into the picture, i.e., place the data in an explanatory relational context, and describe the two animals as waiting for their prey, as playing together, or as cohabiting. Only 2 Nupe descriptions (1 recall) try a similar solution.

Cognition of meaning and relation in this sense may, however, refer not only to figures and events presented in the picture, but to the picture itself *qua* picture. Some of the Yoruba approach their task in this fashion, and attempt to relate the picture as an item in itself to other facts of knowledge and experience. They try, for example, to explain the fact that such and such a picture had been shown; they understand the picture in the sense of their school-book illustrations, i.e. as an invitation to relate, in the manner of a school examination, everything they know about one or the other item illustrated in the picture. Thus in the picture of the horse and groom:

(Y 9) The horse is an animal that lives with us people. We supply it with grass for food, and we ride it for pleasure or something else. We use it as a pack animal, and also for farming. We harness it.

In other cases, the Yoruba seem to be struck by certain peculiar features in the composition of the picture, and try to explain them, by quite an elaborate process of reasoning, as a result of the formal arrangement:

(Y 20) There is a camel with big loads on his back. He stands near a palm-tree. If one does not look carefully one may think he has no head. But he has one. He lifts his head up to the palm, so that the head is level with the palm-tree. That is the reason why we see only the palm-tree. There is a man close to his hind-leg, he is squatting, and there is a man standing in front. These men are men from the East, because you can see they cover their heads with a cloth.

This attitude of explaining and reasoning which deals with the picture as an object in its own right is completely absent among the Nupe.

(4) Exactness

The examples will have revealed already that the cognition of meaning does not correspond in any way to exactness of perception or memory in the sense of fidelity to the original. Among the Yoruba the proportion of meaning-oriented interpretations increases from description to recall, among the Nupe it decreases. That means the recall emphasizes still further the type of interpretation manifest in the description. But the main point here is that imaginary additions also increase and decrease in the same proportion. The recall of the Yoruba is therefore, from this point of view, less exact than the recall of the Nupe. The enumerative type of perception and memory on the other hand is less exact in other ways. The Nupe frequently confuse the pictures in their memory and reproduce in the recall descriptions in which the features of two pictures appear combined. This combination is never an improvement from the point of view of consistency of meaning. There is no evidence for this fusion of memory pictures among the Yoruba. The Nupe also forget pictures completely much more frequently than the Yoruba. The first picture was left out by 1 Yoruba and 4 Nupe, the second by 1 Nupe only, the third by 2 Nupe only, the fourth by 1 Yoruba and 7 Nupe, the fifth by none in either group, the sixth by 1 Yoruba and 2 Nupe. The most interesting figure is the one referring to the fourth picture (the oasis) in which Nupe 'forgetfulness' reaches its maximum. For both groups, however, this picture represents a maximum also of meaning-oriented interpretation. This, I think, stresses an important point with respect to the evaluation of these results. It shows that we cannot simply say that meaning-oriented interpretation assists, or lessens, memory. For the meaning-oriented type of memory, meaning-oriented organization of the psychological material assists memory; for the enumerative type, into which it does not fit, meaning-oriented organization of the psychological material constitutes an obstacle to remembering. In short, the situation proves the impossibility of any simple comparative evaluation of the two types of psychological response in accordance with a common scale of exactness and, accordingly, of intellectual efficiency.

(5) Enumerative response

What I called the 'enumerative' response type does not, however, represent a reaction devoid of all unitary organization or integration. It is enumerative only with respect to the meaning read into, or found in, the contents of the pictures. But there exist other aspects of organization

in Nupe description and recall, which are absent, or nearly absent, among the Yoruba. I have found 3 such principles of organization: (1) The Nupe arrange their material very frequently in a spatial order, working with such descriptive categories as top and bottom, left and right, front and back. This emphasis on the spatial arrangement of the perceptual material seems to correspond to the stress on situational and temporal arrangement which we found prominent in the Nupe reproductions of the narrative. (2) There is the tendency to an integration or consistency in the material of a structural, formal character, such as consistency embodied in symmetric proportion. Imagination is engaged in the service of this specific form of integration, and we find, for example, in the picture of the two leopards that two trees are invented to match the two animals; or we find descriptions such as these:

(N 23) Two cats, white and red, one big, one small, close to two trees.

(N 7) A very beautiful horse, with two men, one standing in front and one standing at the back.

(3) Finally, there is an organizing principle, again working for some form of unification, which can be best described as unity of impression or of emotional tone. The following examples, which illustrate this attitude, refer to the third and fifth picture:

(N 6) A man, he is huge, he wears a big gown, he is an important man.

(N 7) An ugly man, wearing clothes which are not nice looking, holding something in his hand that looks like a snake.

(N 4) This (picture) looks as if one were sowing in all directions, like the sowing of European flowers of reddish colour; that is to say they are not quite red, they are between red and white, there are small ones and thick ones, some are not big, some are on the top and some stand out, enormously big.

This 'impressionistic', emotional attitude is not very pronounced in the Nupe reproductions. It is scattered irregularly, and seems to correspond essentially to the specific emotional response in the story experiment, which we interpreted as the expression of an independent, secondary psychological trait among the Nupe.

VI. CONCLUSIONS

The experiments have shown us a people possessing a highly developed realistic image-art, drama, and a rich, complicated, rationalized religious system, which, in the field of psychological reaction, appears characterized by stress on rational elements and logical cohesion, and by a strongly pronounced meaning-oriented approach to observational data. On the other hand, a people whose culture possesses no such art or drama, whose

beliefs revolve round abstract, impersonal magical principles, proves psychologically of a very different type. It is more detached and inconsequent in observation and memory, enumerative, more sensitive to spatial and temporal arrangement, and stresses unity of situation and emotional tone rather than facts of meaning and rational consistency. The nature of this psychological differentiation clearly indicates that the experiments have done more than merely shown that a diversity in culture is accompanied by psychological differentiation; they prove the existence of an essential *correspondence* between the specific organization of the culture and the traits and dispositions which characterize the psychological make-up or 'type' of the group. Some aspects of this correspondence are quite evident and easily explained in terms of mutual interaction of culture data and psychological traits: for instance, the correspondence between the rationalized religious system of the Yoruba and their intentness on rational connexions and interpretations; or the correspondence between the ornamental, decorative art of the Nupe and their emphasis of spatial and symmetrical order. The nature of other aspects of this correspondence is less obvious, and can be interpreted only in the context of a detailed analysis of the whole culture: for instance, the correspondence between the impersonal, abstract religious system of the Nupe and their schematic, unspecific attitude to moral judgements.

The fact that we are dealing here with two groups which we had to contrast in various ways, may give the impression that I am attempting a dualistic theory of memory or perception, or for that matter, of culture. This is not my intention. Rather, the psychological traits and characteristics which the foregoing analysis has disclosed, seem to integrate themselves into *three* different 'types'—types which are distributed in different proportion among the two groups: first, the dominant type of the Yoruba, rational, logical and intent on meaning-oriented interpretation; second, the dominant type among the Nupe, combining an enumerative approach to observational data with stress on spatial and temporal arrangement; third, a less prominent, secondary, type among the Nupe, characterized by an emotional and 'impressionistic' disposition. Evidently, for a fuller understanding of these psychological types, the investigation must be placed on a broader basis, and our correlations of psychological traits extended over other fields of psychological reaction. But such an extension would go beyond the scope of this paper, in which I could only attempt to outline the general trend of this correspondence between psychological and cultural differentiation.

VII. SUMMARY

1. The aim of this investigation was to study, by means of the psychological field experiment, the correlation between diversity of culture and psychological differentiation.

2. Two primitive groups were studied, two tribes in Northern Nigeria which, living under similar conditions, yet show marked differences in their culture.

3. The experiments were carried out with school boys of the age of 15-18. The selection of the subjects, however, insured that the experimental groups were representative of the culture at large.

4. The experiments examined the repeated reproduction of a narrative, and the description and remembering of a number of pictures. Both the narrative and the pictures represented a material with which the experimental subjects were thoroughly familiar.

5. The results show a marked difference in the responses of the two groups. In the story experiment one tribe laid stress on logical and rational elements while the other showed distinct appreciation of situational facts and connexions of time and place. In the picture experiment the response of the first group was meaning-oriented, the response of the second enumerative and appreciative of spatial arrangement.

6. Between the cultural differentiation and the psychological differentiation there exists close correspondence. The dualism of culture and psychological responses is incidental; the investigation reveals in fact three different psychological 'types' distributed in different proportion among the two groups.

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HINTS FROM PHYSICS AND METEOROLOGY AS TO MENTAL PERIODICITIES

By LEWIS F. RICHARDSON

FRESH interest in mental periodicity has been aroused by the researches of Flugel (1928, 1934) and Philpott (1932).

There is a formal resemblance between psychology and meteorology. In both sciences an enormous collection of observational data has been open to public inspection for a long time. In both sciences the attempt to find order among the miscellany of the observations has proceeded slowly with many failures. In both sciences there are too many circumstances varying simultaneously for the comfort of the mathematician who would like to make a neat, simple theory. Both sciences use correlation coefficients freely. But, judging by the extent to which observations are explained by theories, it appears that meteorology is ahead of psychology, perhaps by the work of a generation.

With this preface, a warning issued by the editor of the *Quarterly Journal of the Royal Meteorological Society* (Sir Gilbert Walker, F.R.S.) may be of interest to psychologists. He (Walker, 1936) "would draw their attention to another duty laid upon him by Dr Jeffreys in his recent book on *Earthquakes and Mountains*. On p. 54, after drawing attention to the two criteria for reality in periods and lamenting the frequency of the errors to which their neglect gives rise, he remarks: 'It is to be wished that editors of journals would make it an absolute rule not to publish papers on periodicities if these criteria are not applied; if the results are not significant they are worthless, and if they are significant opinion is prejudiced against them in advance.' If, as is probably true, ninety-five per cent of the periods announced are non-existent, it will save costs in printing as well as valuable time spent in reading, if such a rule is enforced."

That is the stern rule at which meteorologists have arrived, after half a century's experience of periodicities. On enquiry from Sir Gilbert Walker for particulars of the "two criteria for reality" he kindly sent me the references under his name at the end of this paper.

Turning now to physics, we notice that periodogram analysis (Schuster, 1906) on which the aforesaid dread "criteria of reality" are based was worked out in those decades when the vibrating systems regarded as

typical were those like the pendulum, the spring-and-mass or the inductance and capacitance. When properly stated periodogram analysis is, of course, a set of theorems in pure mathematics, quite independent of any alleged mechanism. Nevertheless it was generally supposed that the periods, when found, were to be explained by a mechanism of the spring-and-inertia type. The general characteristic of such mechanisms is that the period is well defined, not much affected by damping, nor much affected by outside small disturbances. Accordingly one of the best methods for finding the period was to count the number of periods in a long interval of time.

In 1926 B. van der Pol set before us a new type of oscillation which he named relaxational (van der Pol, 1926). It does not depend on inertia, but on capacitance and resistance. Its wave form is characterized by regions where the curvature is much greater than in any part of a sine curve of the same wavelength and same range of oscillation. Its period is subject to outside control to a much greater degree than is that of the spring-and-inertia vibration. The solutions of van der Pol's differential equation,

$$y'' - \epsilon (1 - y^2) y' + y = 0 \quad \dots\dots(1),$$

are of course strictly periodic. But just there the mathematics is a misleading idealization of the facts. Moreover the class of relaxation oscillations is much wider than those represented by that equation. To apprehend the essential distinction between pendular and relaxational oscillations we must attend to the physics. In the rising phase of a relaxation oscillation a capacitance of some sort is continuously filling. At a certain level of fullness the resistance breaks down and a sudden outflow occurs. The process then repeats itself. In each period there is an unstable phase when the capacitance is filled nearly to the breaking level; so that a small external disturbance will cause a breakdown prior to its normal time. In the swing of a pendulum there is no unstable phase, nor does one occur in the electrical oscillation of a condenser and self-inductance. B. van der Pol (1930) has drawn attention to a number of biological oscillations that are relaxational, notably the heart beat. It seems likely that many mental oscillations involve an unstable phase and for that reason their periodicity is variable. The widespread popularity of syncopated music is a sure indication that there is something in the human nervous system that oscillates with rhythm that is not regular.

But, to return to physical illustrations, a neon lamp arranged with a condenser, resistance and battery to flash after the manner of Pearson & Anson (1922) usually flashes with a period that is not steady.

The reason is interesting. If the time-interval since the previous flash has been sufficiently long, the electric current has entirely ceased. The current u will not depart from zero until the voltage ϕ across the lamp exceeds a critical value ϕ_e . The voltage meanwhile is rising in a continuous predictable sweep. But du/dt is proportional to $u(\phi - \phi_e)$. So that, even when $\phi > \phi_e$, the rate of growth of current is indefinitely slow, unless some accidental disturbance, such as incident radiation, gives the current a start. If suitable radiation is abundant, the period is fairly steady; if radiation is very scarce, the period becomes extremely erratic. [Braunbek (1926), Valle (1936), L. F. Richardson (1937)]. *Query*: Do cosmic rays start the release of stored brain-energy and so occasion sudden thoughts?

Again the Abraham-Bloch multivibrator will, when left alone, run with a period steady to 30 parts in 100,000; but it will also follow with an accuracy of 1 part in 100,000 a period differing from its natural period by ± 4000 parts in 100,000 supplied to it by a valve-controlled tuning fork (Dye, 1924). This is in contrast with the behaviour of pendulums. To control a pendulum, the period of the controlling impulse would have to agree much more closely with the free period of the pendulum; or else proportionately more energy would have to be supplied.

We may describe this contrast by saying that, as regards the interval between pulses, relaxation oscillations are docile, whereas spring-and-inertia oscillations are obstinate.

When observational data are to be analysed in the search for relaxation oscillations the established methods of periodogram analysis are unsuitable, for they depend on steadiness persisting over many periods: on action at a distance in time. Instead, following Faraday, we need to study action in a neighbourhood of time. Udny Yule (1927) has made an important step in this direction, although he thought in terms of pendulums. Yule (1927) contrasted two types of disturbance of simple harmonic motion. In the first type a pendulum swings regularly but the observations have random errors. The observed curve is spiky but the underlying period revealed by periodogram analysis is steady. In the second type of disturbance it is imagined for illustration that the observations are made with precision but that boys throw peas at the pendulum from opposite sides. The observed curve is smooth, but the phase and amplitude wander. For the reduction of the latter kind of observations Yule has devised a statistical method which is an approximation to the fitting of the differential equation

$$\frac{d^2y}{dt^2} + 2k \frac{dy}{dt} + \omega^2 y = 0 \quad \dots\dots(2)$$

to each successive small part of the curve.

Yule's data were given at equal intervals of t , the values of y being, say, $y_0, y_1, y_2 \dots y_n$. What he did in practice was to use the method of partial correlation to determine b_1 and b_2 in the regression equation

$$y_n = b_1 y_{n-1} - b_2 y_{n-2} \dots (3).$$

In this way k and ω can be determined.

If instead of (1) the phenomenon is described by van der Pol's equation,

$$\frac{d^2 v}{dt^2} + \alpha (1 - v^2) \frac{dv}{dt} + \omega^2 v = 0 \dots (4),$$

a procedure rather similar to Yule's is indicated, but the regression will not be linear.

If this were done it would of course still be necessary to submit the derived statistics to the test of criteria of reality appropriate to the new method. But by computing in a neighbourhood, instead of over a long time, we should avoid the risk of having a real oscillation rejected as spurious merely because it had an unstable phase.

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PUBLICATIONS RECENTLY RECEIVED

Ability and Knowledge: The Standpoint of the London School. By FRANK C. THOMAS. London: Macmillan and Co., Ltd. 1935. Pp. xx + 338.

We have here the first attempt, of a really comprehensive kind, to make Spearman's principles available to the non-technical reader and to those students who have neither the time nor the capacity to digest the original volumes and articles in which the researches of the London School are locked up. Though the author adopts a systematic, logical arrangement of themes, it is more adapted to the interests of the untrained reader than is Spearman's own treatment. Nevertheless one feels that it might have been more in keeping with the aim of the book, and fitting to the racy style in which the chapters are written, if a less rigid treatment, psychological rather than logical in approach, had been adopted.

The first section deals with individual differences in ability and describes very lucidly the two factor theory. Similarly the next section, dealing with personal variations in ability, is made an opportunity for setting out the quantitative or a-noegenetic principles. Thence one is led, through a section on the birth and growth of knowledge, which treats of the noegenetic principles and education, to a final section evaluating all these contributions in the light of other and opposing psychological 'schools'.

Of this last section it may be said that the author leaps readily to face all manner of real and imaginary objections and criticisms, but that he leaves the reader somewhat in doubt as to the issue of one or two of the most important controversies. The discussion of the arguments of Wilson, Camp, Heywood, Piaggio and Brown, on the indeterminacy of ' g ' is itself somewhat indeterminate, and the description of the controversy with Thomson omits to point out that though the hierarchical correlation table will fit a group factor explanation, not all group factor arrangements will fit in with that hierarchical table which uniquely fits the two factor theory, i.e. that Spearman at least has the balance of probability in favour of his alternative. However, such difficult issues perhaps do not admit of more simplified statement than the writer, by use of metaphors, has given to them.

One is forced to recognize that this book is more than a skilfully prepared 'popularization' of the viewpoint of the London School; it contains appreciable contributions of a novel kind; and since the work has Spearman's approval in a foreword, we may suppose that these additions are not considered inapt. The writer has a gift for perceiving new connexions and for interpreting other theories convincingly in the light of the general theory with which he is concerned. Behaviourism, for example, which for many people seems to lack even the connexion of a straightforward opposition to Spearman's viewpoint, is logically fitted in among the quantitative determinants: "Conditioning has its own sub-subdivision of retentivity." Among the lesser flashes of illumination is an explanation, on the basis of the quantitative laws, of the difficulty, presented as a criticism by Hazlitt, concerning the reasons why one relation rather than another should be perceived between two fundamentals related in many ways. Another valuable clarification occurs in the discussion which shows how the proportions of g and s in a particular activity vary not only for different people and different ages but for the same person at different stages in the learning process.

A chapter in the appendix, applying Spearman's principles to the old problem of transfer of training under the title of "The diffusion of learning effects", is particularly successful, but the most valuable contributions of the writer are of a more general kind—the fruitful union of the factor theory and noegenetic principles, the extension of the principles of conative control and the welding of individual and general psychological principles.

The writer's lively, easy and at times brilliant, style, brings humour and warm life

into a subject which, even among psychologists, has sometimes been stigmatized as coldly statistical, humanly meaningless and detached from real problems. This successful treatment is largely due to ample and apt illustrations from everyday life, and a reading so wide that it is bound to catch the reader's sympathies at some point of common interest. For the psychologist as such these illustrations may be excessive and clog the flow of argument, whilst for some people they may have too off-hand and autobiographical a flavour, but for the type of reader whom the writer has in mind they are undoubtedly successful.

For Worker's Educational Association Classes, teachers and students taking psychology as a subsidiary to other subjects, this book certainly meets, very squarely, an urgent need. From whatever other angles it may be subject to criticism, it admirably fulfils the purpose, beset by formidable difficulties, of providing for a large body of readers, a lucid, fair and interesting account of the work of Spearman's school and its application to practical problems.

R. B. C

The Fight for our National Intelligence. By R. B. CATTELL. London: P. S. King and Son, Ltd. 1937. Pp. xviii + 157. 8s. 6d. net.

The pages of this book in which Dr Cattell describes his experiment are admirable for their clarity and for their scientific detachment. What he did was to measure the intelligence of 10-year-old children in a typical urban and a typical rural community and to find that the average intelligence of children in smaller families was higher than that of children in larger families. Intelligence is inherited to some extent (though to how great an extent is a matter of controversy), and the conclusion therefore emerges that the intelligence of the community as a whole is declining. In previous work Dr Cattell has shown that the intelligence level of the people in better paid occupations is greater than that of people in the lower paid, and it is known that individuals in the higher paid occupations on the whole have fewer children than those in the lower paid. Furthermore there is no longer, as there used to be, a marked differential infant mortality rate, which in the past may have corrected the present tendency towards a declining level of intelligence of the population as a whole.

The outlook, as Dr Cattell sees it, is gloomy. Most of the book is devoted to describing the effects of the decline in language which is often more graphic than detached. But in spite of this defect in the text the serious reader may obtain a wealth of information from the footnotes, the footnotes to footnotes, and the footnotes to footnotes to footnotes, though even these suffer occasionally from too rapid revision.

J. M. B

Stillings pseudo-isochromatische Tafeln zur Prüfung des Farbensinnes. English Edition translated and edited by JAMES DREVER. Leipzig: Georg Thieme. 19th revised edition. Pp. 15 + 34 plates. 22s. 6d. net.

Everyone who is interested in colour vision, whether as medical man, physiologist or psychologist, will welcome this revised edition of Stilling's now world famous tests for colour perception. This new edition, the nineteenth, has been edited by Prof. Hertel, of Leipzig, and differs in several ways from those which preceded it. Of the older editions only those plates which have been adequately standardized on normal subjects are here retained. New plates, some of which have been made possible by reason of recent technical progress in printing, have also been incorporated; these too, it is claimed, have been properly standardized. As in the eighteenth edition, plates which render the detection of blue-yellow confusion possible have been inserted. It is inevitable that these should not give quite such clear results with normal subjects as do those designed to detect the far more frequent, and somewhat more important green-red defect. As far as may be observed, however, it is readily possible to detect relatively slight deviations from normality here, and these tests are, of all simple methods available, unique in this respect.

Prof. Hertel has extended Stilling's suggestion that the subject, in addition to

reading the numbers contained in each plate, may be asked to indicate dots of like colour, by including in this edition four plates specifically designed for this purpose. Such a refinement of testing as this may, in general, only be applied to those patients or subjects to whom considerable time can be devoted. They provide, however, a useful extension of the normal test for special cases.

In this edition the plates are made up not in book, but in 'multiple-folder' form. Whilst this may have some possible advantage in reducing the time necessary for the test, it would appear also to have considerable disadvantages; for, without special and somewhat awkward precautions on the part of the tester, it is possible for the subject of the test to see several (at least four) of the plates at once. It would seem that this scheme, by introducing an additional variable into the test situation, is of no real advantage.

A word of congratulation and thanks should be given to Prof. James Drever, whose excellent translation of the preface and instructions has so materially enhanced the usefulness of the English edition.

A. F. R. S.

Polychromatic Plates for Color Sense Examination. By E. B. RABKIN.

Kharkov; State Medical Publishing Board. 1936. Pp. 40+20
coloured plates. Price 30 Roubles.

This series of tests for colour defect issues from the Laboratory for Colour Vision of the Hirshman Memorial Ukrainian Central Ophthalmic Institute, of which Dr Rabkin is the Director. The tests are composed of coloured figures on coloured backgrounds such as are found in the Stilling and the Ishihara tests. A number of the plates contain also 'hidden' figures, which are visible only to the dichromate. Triangles, circles and squares appear on some plates instead of figures, and so can be used for testing children and illiterates. A diagnosis can be made from the tables of dichromasy in general, and of protanopia, deuteranopia and tritanopia in particular. The author claims, too, from the tests that "the simpler forms of anomalous trichromasy" can also be detected. Plates are added to detect simulation.

The series itself has been tested out in the Experimental Ophthalmic Clinic of the Institute of Experimental Medicine at Kharkov, and has proved successful. It seems, if one can judge from a preliminary survey, to give satisfactory results, and can be recommended. The book contains detailed and clear instructions in both Russian and English, although the English translation contains some curious phrases and sentences, and the spelling is not always of the best.

M. C.

Studies in the Psychology of Reading, Vol. 1. Edited by JOSEPH TIFFIN.

University of Iowa Studies in Psychology No. XXI. Edited by
CHRISTIAN A. RUCKMICK. Psychological Monographs, vol. XLVIII,
No. 3 (whole No. 215). 1937. Pp. vii+149.

This volume contains studies of eye movements in reading by Irving A. Anderson, Donald E. Swanson and Grant Fairbanks. The conclusions arrived at are: that eye movements do not govern reading ability, but that variations are produced by conditions which control the central processes in reading, e.g. (1) changes in the difficulty of the reading material, (2) changes in the attitude or 'mental set' produced by alterations in instructions, (3) differences in general intelligence. Thus attempts to improve reading ability by training eye movements are useless—a conclusion which was reached many years ago, but apparently is still too frequently ignored. A close correspondence exists between silent and oral reading; when slow rate and poor comprehension are shown in the one, they will probably appear in the other, since both are due to errors central in origin. But the better the reader, the greater the discrepancy between eye movement performance in silent and oral reading, i.e. poor readers tend to resort to the mode of attack characteristic of oral reading, while the mature reader has been 'weaned' from oral reading habits.

The number also includes a description of an eye-voice camera by Joseph Tiffin and Grant Fairbanks, and a study of the maturation of certain visual functions connected with binocular fusion, by Guy W. Wagner.

Nouveau Traité de Psychologie. Par GEORGES DUMAS. Tome v. Fascicules 3 and 4: *La Croyance.—La Psychologie de la Raison.* Par H. DELACROIX; *Le Temps et les Souvenirs.—La Rêve et la Réverie.* Par H. DELACROIX. Paris: Félix Alcan. 1936. 20 fr. each.

These two publications continue Dumas' comprehensive new handbook. As in the case of the earlier publications of the same general treatise, they are suitable rather for the advanced student than as an introduction to psychology. Each part here noticed contains two essays of which one is very much fuller and better than the other. The discussion of Belief in the one book and of the Dream and Reverie in the other are not very satisfactory. They are brief, highly selected and will serve their main purpose in introducing the reader to more extended treatments elsewhere. It seems especially a pity that a more thorough and positive consideration of Belief could not have been included. On the other hand, the exposition of the psychology of intelligence and thinking processes is good. It is very well documented, and such criticism as is introduced is usually penetrating and sound. Similarly the discussion of various problems of Recall, following a rather brief account of the psychology of temporal perception, is very good indeed. Bibliographies are attached to the sections on belief, on memory and on the dream. They make no pretence to be complete and that is all to the good; but it is more than a little difficult to discover by what principles the selection has been made. In the hands of a student who already knows a good deal about the psychology of the topics dealt with, these two publications should be of considerable service.

Le Temps. Par M. SOURIAU. Paris: Félix Alcan. Nouvelle Encyclopédie Philosophique. 1937. Pp. 178. 12 fr.

The author covers an enormous field, and writes in a rather epigrammatic, slightly jerky, fashion. He begins by inviting his readers to contemplate the whole universe, and proceeds to a rapid consideration of the concept of time as it is used in very nearly every field of knowledge within which it is relevant. One can get a lot of enjoyment out of the discussion, but the psychologist will probably often find himself out of his depth, and on the whole he will not discover very much that is relevant to his own problems.

The Nature of Human Nature. By ELLSWORTH FARIS. London: McGraw-Hill Publishing Co. 1937. Pp. xii+370. 21s. net.

It is a little difficult to present a brief and fair evaluation of this book. Its material has been collected from a great many different sources, and most of it has been published before. The full title of the book is *The Nature of Human Nature and other Essays in Social Psychology*, and this accurately describes both the character and the scope of the volume. Anything can be included which has a bearing upon social problems, and most of the different chapters are really brief discussions complete within themselves. Hence the reader can open the book and read wherever he likes. He will generally find something attractive and suggestive, rarely find anything thoroughly worked out. Most of the discussions are on a very general level, and concern the use of ideas rather than the exploitation of definite methods of empirical research. The essays are grouped under the following five headings: Group and Person; Conduct and Attitudes; Sociology and Education; Sociology and Ethnology; The Sociology of Racial Conflict. While this is in no sense a text for students, or a systematic treatise for the specialist, it is a volume which can be read with much pleasure and profit. Every teacher of psychology knows

that there come times when his better students need to be directed towards reading that can be undertaken without the stress and strain that often accompany specific preparation for examinations. When that sort of time comes round this would be a very useful book to recommend.

Primitive Behaviour. By WILLIAM I. THOMAS. London: McGraw-Hill Publishing Co. 1937. Pp. ix + 847. 30s. net.

This is described as *An Introduction to the Social Sciences*. It belongs to the type of treatise which gathers its material from every possible source and covers a great many different fields of behaviour in a great many different parts of the world. The actual material itself is well selected and set forth clearly, always with adequate source references. The reader will, in fact, find this book useful, not because it presents him with any consistent sort of picture of life in a primitive community, or even of a hypothetical life in a hypothetical primitive community made up of the common features of many actual groups, but because it presents a most excellent introduction to many specialized fields of study. All the primary departments of behaviour: habit, language, kinship customs, incest, descent, sex, intimacies and avoidances, government, law, and several others are dealt with in detail. There is a special study of the diffusion of culture patterns; there is also some discussion of general methodology, exemplified later by a consideration of Bantu culture. Finally there is a chapter dealing with the relative mental endowment of races, but this is almost entirely in general and descriptive terms, and does not amount to very much. The bibliographies are full and very good indeed. If a general book, dealing descriptively with all the fields of primitive behaviour, is really required, this is an extremely good piece of work. All the same, a reader who goes right through the volume from beginning to the end will probably have amassed far more detail than he can properly manage, and he will have to look elsewhere for much direction concerning the ways in which he can best utilize the knowledge he has acquired.

Bird Behaviour: A Contribution based chiefly on a Study of the Black-Headed Gull. By F. B. KIRKMAN. London: T. Nelson and Sons. 1937. Pp. xv + 232. 7s. 6d. net.

In this important contribution to the study of bird behaviour Mr Kirkman has adopted, as the subtitle suggests, the method of observing in detail one (rather large) part of the behaviour of a single species. For about thirty years Mr Kirkman has watched black-headed gulls in their gulleries during the breeding season. His observation tent has become such a familiar feature of the landscape at each breeding season that the gulls ignore it, and are not disturbed by the fact that their actions are being watched through field-glasses, timed, noted down in detail, and—often—photographed. Sometimes Mr Kirkman has abandoned the role of passive observer, emerging from his tent to substitute a cricket ball for an egg, to place a chick in a strange nest, or to move an egg some little distance from the nest—only to return to his tent to watch the efforts of the birds to deal with the new situation which he has created.

The book contains a mass of observations and experiments, carefully arranged, on social life, behaviour to neighbours and intruders, nest-making, incubation and attempts to incubate other objects which have been substituted for the eggs, the feeding of the hen and young, retrieving eggs which have been displaced, and emotional expressions associated with hostility, fear and sex. It is impossible to summarize the observations in a short review, but there are several features of the book which are of special psychological interest. Among these are the many experiments on the acceptance of egg substitutes and on the capacity to roll back to the nest eggs which have been displaced, and the careful study of the conditions which arouse hostility in gulls and of the different ways in which hostility is expressed.

Mr Kirkman is himself interested in trying to form from his studies a picture of the mental life of gulls, and in his general approach to the problem of mind in animals he follows Lloyd Morgan rather closely. The gulls emerge from the examination without very much credit for higher mental processes.

A number of excellent photographs add to the vividness of the descriptions of behaviour. To those interested in any of a large number of aspects of bird behaviour the book will be a very valuable source of material.

Die Lehre vom Charakter. Von ROBERT HEISS. Berlin und Leipzig: Walter de Gruyter and Co. 1936. Pp. vi+273. Geb. Rm. 8.80.

To the English reader this book must possess very considerable interest, for it gives, within a reasonable compass, a very good account of the development of character study which has taken place during the last few years in Germany. It contains three main parts: the first deals with the inherited and permanent basis of character, the second with general problems in the development of character, and the third with the properties which character, regarded as a more or less completed structure, may be said to possess. The whole discussion proceeds upon a very general analytical level. There are no case histories, there is no mention of questionnaires, or rating-scales, or inventories, or measuring instruments of any kind whatsoever. There are plenty of general schemes, concerning different categories of temperament, for instance, or different general formative forces of character development. One gets the impression that behind all is a mass of patient and pertinent observations, but that all the observations are subsequently pared away and the only thing that is presented is the very general kind of conclusion to which they may lead. It looks in the end as if character resolves itself into a number of properties and the relations between them, but as if, somewhere hidden among them, is a veritable kernel of personality, a driving force, and not a name for some special sort of psychological organization. But precisely what this kernel is does not become very clear. The bibliography which is added is well selected, and some very good brief notes are given, with many of the entries, but beyond a reference to a book by A. Fouillée published in 1895, there is no reference to any work outside of Germany.

Die Grundlagen einer Milieupsychologie: Milieu und Kind. Von J. O. VÉRTES. Leipzig: J. A. Barth. 1936. Pp. 37. Brosch. RM. 2.25.

Individual behaviour may be regarded as due to an interplay between an objective milieu-projection and a subjective milieu-projection. In this monograph the author takes this as his basic assumption, shows what kinds of definition of behaviour problems it may lead, indicates the methods of solution which are most in line with his fundamental hypothesis, and very briefly suggests solutions in some instances. The actual exposition is very clear and definite in phrasing, and yet the notion of 'subjective Milieuprojektion' does not become any too easy to understand. Apparently it consists of factors of which the individual is normally unaware—perhaps must be unaware—which do not change themselves, but which determine the specific ways and directions in which the 'objective Milieuprojektion' are active. It is upon well-adjusted relations between these two projections that healthy conduct depends, and it is the business of society, the school, the family to secure such well-adjusted relations. Dr Vértés gives a number of original and interesting illustrations, and briefly sketches some of the outstanding problems for future research.

La Conscience et le Corps. Par R. RUYER. Paris: Félix Alcan, Nouvelle Encyclopédie Philosophique. 1937. Pp. 143. 10 fr.

The book is distinctly worth reading. It is written with spirit, humour and 'go'. It displays a great amount of learning carried very lightly and pleasantly. If it does

not issue in anything very new or more profound than has been achieved before that is more, perhaps, the fault of the topic than of the author. It does give a very excellent account of a great variety of views, philosophical and experimental; it does criticize them fairly and well, and it does state clearly and emphatically the position that whatever else is done about the matter consciousness has got to be taken at its face value as a genuine fact with which any biological approach to the problems of human life must reckon. First 'The Doctrines' are expounded and criticized. Then various attempts at a solution of the problem of the relation of mind and body are passed in review, generally with many illuminating remarks. *Gestalt* views receive much attention and in general are approved with reservations. The final chapter called "Conscience et Organism" is much the shortest, and naturally enough most readers will probably find it the least satisfactory. As a critical survey, always guided by a serious attempt to reach positive conclusions, the book is excellent.

L'Imagination. Par J. P. SARTRE. Paris: Félix Alcan, Nouvelle Encyclopédie Philosophique. 1936. Pp. 162. 10 fr.

Although this book is oriented rather towards metaphysics and epistemology than towards empirical psychology, it contains much that the empirical psychologist should carefully consider. The general problem is: if all mental facts are to be regarded as somehow integrations or syntheses, how on earth can we reconcile images with this, since they stubbornly maintain their character of relative isolation and discreteness? The author does not really solve this problem, but he has many excellent things to say about it, especially in the chapter which deals with the efforts made by psychologists to find some adequate positive methods for investigating images. In the end he comes to the view that images must not be regarded as *in* consciousness, but rather as being a certain kind of consciousness. "The image is an act and not a thing. It is consciousness of something." Still nobody can discard entirely the notion of structure in regard to images, only that notion becomes phenomenological. Here comes a very interesting discussion of some of the views of Husserl. To the thorough phenomenological analysis of 'structure images' the author proposes to devote another treatise.

Bewusstseinstypen. Von J. H. VAN DER HOOP. Bern: Hans Huber. 1937. Pp. 375. Fr. 15; oder RMk. 9.

The book is written from the point of view of a practising psychotherapist. So far as the typology of the book goes the primary influence comes from Jung, while the author acknowledges also his debt to the phenomenology of Husserl and Heidegger. Dr van der Hoop had had a wide psychopathic experience, including a period at the Boston Psychopathic Hospital, and he has utilized this well so as to produce a book of very great general interest, though it is perhaps a little on the long side. The first part of the volume deals with "Bewusstseinstypen und Psychologie". The general scheme of the treatment is to apply Jung's type classification first to instinct types, then to intuitive types, then to thought types and finally to feeling types. After this, various complications of type classifications are considered and the relations of the types are discussed. While there is some reference to the approaches of Heymans and Wiersma, of Kretschmer, Klages, Stern, Ewald, Hoffmann, Kronfeld and Spranger, these are not dealt with at all fully. In each chapter, after a short consideration of the significance of instinct, intuition, thought, or feeling in general psychology, the rest of the discussion proceeds along the line of a description of extrovert and introvert types. The most interesting and original chapter concerns "Das Denken und die Denktypen". The second part deals with "Bewusstseinstypen und Psychiatrie", and after a general discussion of types and complexes in abnormal behaviour, considers in detail types and neurosis, types in relation to pathology in general, and types and psychoses. A third part is concerned with a more philosophical discussion, and turns mainly on a critical consideration of individuality and its significance. The volume is unquestionably one which any psychologist who is interested in the general notion of types, and of methods of investigation connected with this notion, should consult.

The Unconscious through the Ages. By G. K. SABNIS. Reprinted from the *Journal of the University of Bombay*, vol. iv, Part IV, 1936; and vol. iv, Part VI, 1936. Pp. 1-44, 1-38.

After a brief exposition of psychological and psycho-analytical views about unconscious mental process the author proceeds to a somewhat rapid study of the notion of unconscious in ancient and modern periods. In the ancient world the thinkers discussed are Pythagoras, Socrates, Plato and Aristotle; in the modern period Leibnitz, Herbart, Schopenhauer and Hartmann. Notes are added on the views of Samuel Butler, Bergson, F. W. H. Myers, and the whole historical treatment is then summed up. Psycho-analytic theories are then approached again and some criticisms put forward. Mr Sabnis considers that Leibnitz was the first person to recognize the significance of the Unconscious in the modern sense. There is a general distinction between philosophical and psychological notions of the Unconscious, though they are related. There is a sharp distinction between the psycho-analytic conception of the unconscious and that of classical psychology. Though Freud's dualistic notions are often extremely difficult to understand, they have an undoubted practical value and have vitalized modern psychology.

Why We Do It. By EDWARD C. MASON. London: Henry Kimpton. 1936. Pp. 177. 6s. net.

The author says that his book is "not presented with a desire to educate the psychologist, the psychiatrist, or the physiologist. It is modestly presented with a knowledge that some of the thoughts contained have helped individuals to orient themselves and, also, aided others to seek the services of some properly qualified psychiatrist." Dr Mason has, in fact, set out to discuss briefly, and in a popular style, some of the main difficulties of behaviour as they are encountered in actual life. On the whole he has done his work reasonably well, though here and there he will be too technical for the general reader and not technical enough for the specialist. The book does not pretend to possess any particular originality, and though it is quite a good one of its kind there are perhaps more than enough of the kind published already.

War Dance. By E. GRAHAM HOWE. London: Faber and Faber, Ltd. 1937. Pp. 315. 7s. 6d. net.

The author of this book is concerned to show how the process of thinking in terms of wish fulfilment creates for us the illusion of a world in which what we think to be 'right' is always in the ascendant. When we cannot ignore everything that runs counter to this fantasy, we endeavour to eliminate it aggressively. Such aggressiveness in the individual, multiplied a hundred-fold in the community, leads to the attempt to escape anxiety about war by eliminating aggressively all differences between communities which may lead to war. If the individual recognizes that he cannot always understand or obtain what is 'right' for him, but must also accept what is unpleasant, and may also seem 'wrong', he may then adapt himself to those natural differences between individuals which otherwise lead to conflict. There is no fundamental basis for lasting peace between nations apart from such a willing acceptance of the differences between different people's notions of 'right' and 'wrong'.

This theory is both interesting and important. There is little doubt that its truth could be demonstrated in the lives of individuals; though unfortunately Dr Graham Howe quotes no actual case histories supporting his thesis (the dreams quoted are more suggestive than conclusive). The generalization to the case of differences between communities and nations is harder to substantiate, and would necessitate for its proof a study of social psychology, for which at present we possess neither the knowledge nor the technique. But Dr Graham Howe's object has been to put forward an hypothesis rather than to provide a final proof. Though his method of exposition is somewhat difficult to follow, there is little doubt that it will provoke the reader to puzzle for himself over this most vital problem.

New Light on Delinquency and its Treatment. By W. HEALY and A. F. BRONNER. New Haven, U.S.A.: Yale University Press. 1936. Pp. vii + 222. \$2.00.

From the mass of semi-popular literature on delinquency which has appeared in the last two or three years, this book stands out as a real contribution to the scientific study of child-guidance cases. It is a logical continuation of that study beyond the generalizations established by the classical enquiries of Burt and Healy, in the particular direction of comparing delinquents with a control group of non-delinquents in the same families. By this technique certain environmental differences are ruled out, and the field is narrowed for a more intensive study of personal and developmental factors which have been overlooked in previous studies.

It is unfortunate that so well-conceived a research should have been limited, presumably on grounds of expense, to so few cases—133 families—that the majority of the indications are not statistically reliable. It is almost equally regrettable, that, apart from intelligence tests, no objective criteria, e.g. regarding temperament or attitudes, were employed. For the finding that the delinquents were most strongly differentiated from the controls by the number and intensity of disturbing emotional experiences might, along with some other generalizations, be suspect, were it not for the reputation of the writers and the fact that they are clearly free from having any axe to grind in the form of a preconceived theory. The repeated observations of 'over-active personality' among the delinquents is in striking agreement with the objective test finding in this country that high 'fluency of association' is unduly frequent among delinquents.

Psychotherapists in child-guidance clinics will welcome the practical basis offered for a division of cases into three categories according to goodness of prognosis, for too much time is often spent in attempts at short treatment of delinquents whose delinquency is unlikely to respond, as Healy's follow-up studies show.

Though the matter of this research is decidedly such as to interest the research worker or practitioner, the style and the introductory chapters seem designed for the general reader, with a resultant loss in technical exactness and pith.

The Growing Child and its Problems. Edited by EMANUEL MILLER. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1937. Pp. xiii + 231. 6s. net.

It is very difficult to create the effect of a coherent whole from a book made up of essays by different authors, no matter how competent each may be. This book contains essays on Play, by Gwen E. Chesters; Educational Guidance, by Constance Simmins; The Adolescent Girl, by Laura Hutton; Personality Deviations, by Paterson Brown; Neurosis in School Children, by C. L. C. Burns; and Problems of the Growing Child, by Emanuel Miller. Each essay contains a number of interesting and no doubt instructive remarks on how to treat children, particularly difficult children. But no complete picture of the development of the child can be obtained. One is left with an uneasy glimpse of the anxious parent or teacher searching through the book to find which aspect of abnormality there presented will best fit his or her particular problem child. A more comprehensive treatment of the subject by a single one of these authors would leave fewer *lacunae*. Dr Hutton's essay on the adolescent girl is least unsatisfactory in this respect.

Psychologische Diagnostik: Eine Einführung für Psychologen und Erzieher. Von RICHARD MEILI. München: Ernst Reinhardt. 1937. Pp. 256. Brosch RM. 6.

This is a general handbook primarily concerned with the use of tests for educational and other practical purposes. The bulk of it is concerned with a discussion of tests of intelligence and of specific psychological functions of educational significance. Large

numbers of tests are described, their general results set forth, and there is quite a good practical chapter on the analysis of the tests and on methods for their administration. The general technique of psychological diagnosis in the school is considered, and there is some account of specific tests for various more practical performances. A chapter is added on statistical methods, and there is a selected bibliography. On the whole the book gives a good summary of the present position. The author has read widely, in French and English as well as in German, and marshals his knowledge with skill and success.

L'Intelligence et la Pensée de l'Enfant. Par JEAN BOURJADE. Paris: Félix Alcan, Nouvelle Encyclopédie Philosophique. 1937. Pp. 163. 10 fr.

There are four chapters in this book. The first deals with Child Psychology and General Psychology; the second with the Nature and Laws of the Intellectual Development of the Child; the third with the Intelligence of Children in relation to Practical Problems; and the fourth with Interpretations of the Intellectual Development of the Child. Throughout the main effort is to seek and to expound generalizations concerning the character and significance of the development of children. The author shows a very wide and accurate knowledge of a great mass of fairly recent research of the more general character; somewhat less knowledge and appreciation of more technical and experimental advances. Anybody interested in general notions applied to a study of the growth of intelligence can be recommended to study this excellent and clear survey.

Psychology of Adolescence. By LUELLA COLE. London: George Allen and Unwin. 1937. Pp. xvi+503. 12s. 6d. net.

Here is a book which can be very definitely recommended. The author has made an extensive survey of a great mass of literature on the general psychological and behaviour problems of adolescence, and she presents a very readable and well-illustrated study of her results. In the main, and to its very great advantage, the book deals with properly attested facts. At the same time these are skilfully selected and related, so that a genuine picture of adolescence is achieved, and not merely a welter of disconnected detail. It is further a point in the volume's favour that 'normal adolescence' is dealt with first and at some length, and so the one-sidedness of a good many adolescent studies is avoided. The discussion of deviations from the normal is very sane and sensible.

Dein Kind: Wegweiser für Eltern und Erzieher über die moderne Kindererziehung auf psychologischer Grundlage. Von BÉLA SZÉKELY. Wien und Leipzig: Franz Deuticke. 1937. Pp. 223. M. 4.

This book discusses in a fairly simple manner most of the problems which current psychology, especially of the more or less psycho-analytical kind, frequently regards as of greatest importance in regard to the upbringing of children. In style it is excellently clear and comprehensive; it burks no question, but attempts no sensationalism. Although it is definitely practical in its aims, the author does not hesitate to attempt to give all the advice tendered a background of psychological theory and in this is decidedly successful. There is not very much reference to actual investigation, and little to give the anxious parent information as to whether his or her own child is developing in a way characteristic of most children or not. But as a guide for the somewhat theoretically minded parent—provided it does not make him unhappily anxious—it should be of definite service.

Individual Diagnosis: A Manual for the Employment Office. By N. W. MORTON. London: Oxford University Press. 1937. Pp. xvii+123. \$1.25.

This book, which is No. 6 of the McGill Social Research Series, is a summary of the methods at present available in work on vocational guidance. An account is given of the principal tests now in use; a brief description of each is given together with norms where these have been published. The author stresses the value of the personal interview in vocational work, and discusses the relation between test results and the material obtained from such interviews. A brief chapter is included on the statistical analysis of test data. Several case studies are given. The book should prove a useful handbook for workers in the vocational field.

The Educational Guidance of the School Child. Under the authorship of H. R. HAMLEY, R. A. C. OLIVER, H. E. FIELD and SUSAN ISAACS; Foreword by Sir PERCY NUNN; Introduction by K. STRUCKMEYER. London: Evans Brothers, Ltd. 1937. Pp. 122. 3s. 6d. net.

The suggestions on child study contained in this book are put forward in connexion with a scheme for recording the characteristics and achievements of school children, from their entry into the infant school. The scheme has been evolved through use and experiment by the Teachers' Advisory Committee of the Wiltshire Education Committee. Three types of record card are used, one for the infant on admission, one for him after a year in the infant school, and one for the junior and senior school child. The child is rated for intelligence (and also tested for this), home circumstances, physique, and for various traits of character and temperament; various descriptive adjectives for the latter qualities and for the child's interests are also suggested. School attainments are recorded; and on an additional sheet are entered any measures taken to deal with special difficulties. The book not only describes with admirably clear and practical detail the various aspects of the child to be observed and recorded, but it also treats the conclusions which the teacher can draw from these observations as to the child's total personality and general make-up, and the methods of guidance to be adopted in each individual case. The avowed object of keeping these records is that the teacher should be encouraged to observe, understand and treat each child as a separate individual who requires to develop his character in the way best suited to him. This objective and the methods of attaining it are admirably stated and described; one must hope that more and more teachers may be allowed time and opportunity to achieve it.

The New Era in the Junior School. By E. B. WARR. Contributions to Modern Education. Edited by SUSAN ISAACS. London: Methuen and Co., Ltd. 1937. Pp. vii+136. 3s. 6d. net.

Miss Warr is concerned to stress the fact that the education of children in the junior school, aged 7-11, has hitherto received too little attention. It is recognized that the infant school child and the adolescent boy and girl are passing through difficult stages, and much experiment and reform have succeeded to some extent in adapting the educational system to the needs of these children. But the junior school child, though active, eager and on the whole emotionally stable, also needs a special adaptation of the system to ensure that his physical and mental development shall take place along the best lines; and that his natural endowments shall not be wasted or destroyed by confining him to 'memory work' and the 'three R's'. The problem is to give full scope to his natural interests and spontaneous eagerness, while at the same time grounding him in the necessary 'basic skills', and developing in him habits of work and application. Miss Warr believes that this can be done by using modified forms of the 'project'

method and Dalton plan, and describes how this has been carried out in practice in a private school for children of superior intelligence. How far such methods are attainable in the ordinary primary school is very hard to say. But the clear and intelligent treatment which Miss Warr gives, in the first part of her book, of the 7- to 11-year-old child's qualities and needs, will do much to convince the educationist of the necessity and practicability of some such method. The said educationist should not hesitate to obtain this excellent and moderately priced book.

Learning and Teaching: An Introduction to Psychology and Education.

By A. G. HUGHES and E. H. HUGHES. London: Longmans, Green and Co. 1937. Pp. x+450. 7s. 6d. net.

This introductory text-book in educational psychology is designed primarily for training-college students, but is also intended for school teachers who wish to keep in touch with modern developments in the subject. It contains a comprehensive account of the more elementary aspects of child psychology; of the development of the child, its reactions to teaching, and its maladjustment; and of the psychological theory of education. It will be useful to the student in so far as it incites him to further study of and reflexion upon these subjects. But he should not assume that he has obtained a very thorough knowledge from this somewhat diffuse and uncritical exposition.

Theory and Art of Mysticism. By RADHAKAMAL MUKERJEE. London:

Longmans, Green and Co. 1937. Pp. xvi+308. 15s.

This book is a useful and interesting descriptive account of mysticism. As an Indian scholar, the author is particularly well qualified to deal with Eastern mysticism. Its main subject is the various mystical traditions and methods of Hinduism, but it deals also in an unusually sympathetic manner with the Buddhist tradition of mysticism. Although he welcomes the scientific study of religious mysticism, the author's point of view is rather that of the religious believer than of the scientific critic. The usefulness and interest of the book to the Western psychologist is much increased by the number of illustrative quotations from mystical writers.

An Examination of Logical Positivism. By J. R. WEINBERG. (Inter-

national Library of Psychology, Philosophy and Scientific Method.)

London: Kegan Paul, Trench, Trubner and Co., Ltd. 1936. Pp. vii +311. 12s. 6d.

The Logical Syntax of Language. By R. CARNAP. (International Library

of Psychology, Philosophy and Scientific Method.) London: Kegan

Paul, Trench, Trubner and Co., Ltd. 1937. Pp. xvi+352. 25s.

Alarming as the technical apparatus of modern logic and theoretical linguistics may appear, those interested in the psychology of language cannot afford to ignore recent work in this field. Dr Weinberg gives an adequate account of the development of the subject from Wittgenstein's application of the methods of the Frege-Russell School to the problems of language and meaning up to and including the work of the *Wiener Kreis*. The appearance of an English translation of Carnap's formidable work materially lightens the task of studying the later phases of the subject in the original text.

L'Illusion Philosophique. Par JEANNE HERSCH. Paris: Félix Alcan. 1936.

Pp. 204. 10 fr.

La Conception Inductive de la Vie. Par ENZO LOLLI. Traduit de l'italien par Mme D. DE VESME. Paris: Félix Alcan. 1937. Pp. 126. 15 fr.

L'Instinct, ses causes physiques, sa base organique et sa psychologie. Par CÉSAR PORTO. Paris: José Corti. 1936. Pp. 286. 15 fr.

Metodi Compiti e Limiti della Psicologia nello Studio e nella Prevenzione della Delinquenza. FR. ANGOSTINO GEMELLI. Milan: Pubblicazioni della Università Cattolica del Sacro Cuore. Vol. IX. 1936. Pp. xiv + 155.

La Obra Psicologica de Radecki (1910-1935). ALFREDO CÁCERES. Montevideo: La Sociedad de Estudios Psicológicos. 1936. Pp. 119.